# STUDIES

IN THE

# HISTORY AND METHOD OF SCIENCE

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EDITED BY

CHARLES SINGER

OXFORD

AT THE CLARENDON PRESS

1921

Mosaic from Populonium (Etruria). Probably 1st Cent. A.D.


Now in the Victoria and Albert Museum.

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VOL. II



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TO THE MEMORY

OF

WILLIAM OSLER

# PREFACE

The first volume of this series appeared in the autumn of 1917. The editor was unable to see it through the press owing to his absence from England on military duty. A Preface was accordingly provided by Sir William Osler, who, unhappily, has not lived to see the growing success that has attended the ideas he expressed there with so much force, and towards which he contributed life-long thought and effort.

The volume was received with an approval that far surpassed the hopes of its editor and the issue was rapidly exhausted. In the present collection an endeavour has been made to avoid some defects inevitable in the earlier volume. The undue prominence given to mediaeval studies will be found in part corrected and care has been taken to give more space to the evolution of the mathematical and exact sciences, though the balance is yet far from being fully redressed.

Among the many recent signs of public interest in this country in the History and Philosophy of Science special note must be made of the systematic course in the subject now organized at University College, London. The work is under the general direction of Dr. A. Wolf. Professors Sir William Bragg, Flinders Petrie, W. M. Bayliss, L. N. G. Filon, J. P. Hill, F. G. Donnan, E. J. Garwood, T. L. Wren, D. Orson Wood, and the Editor of this series are taking part in it. The course at Edinburgh in the History of Medicine, initiated by Dr. J. D. Comrie, has attracted a large and annually increasing number of students. Mr. A. E. Heath, Senior Lecturer on Education at Liverpool University, is giving systematic instruction there on the History of Science, and the Editor of this series has become Lecturer in the History of Medicine at University College, London, and University Lecturer in the History of the Biological Sciences at Oxford. Great service has been rendered by Mr. F. S. Marvin, who has not only done much to popularize the

History of Science, but has sought to integrate this important aspect of human development in the historical instruction of schools.

Considerable literary activity is also being exhibited. The English translation of Aristotle under the general editorship of Mr. W. D. Ross has continued to appear, as has also the much-needed publication of Roger Bacon's works under the editorship of Mr. A. G. Little, Mr. Robert Steele, and Dr. Withington. Sir Arthur Hort has rendered the *History of Plants* of Theophrastus into English, and a similar service has been done for Galen's short treatise *On the Natural Faculties* by Dr. A. J. Brock. A valuable work in a different department is Sir Norman Moore's monumental and learned *History of St. Bartholomew's Hospital.*

All historians of science look forward with lively interest to the appearance of Sir T. L. Heath's *History of Greek Mathematics*, Professor Dobson and Mr. Brodetsky's version of Copernicus, Mr. W. H. S. Jones's edition of Hippocrates, Professor William Wright and Mr. Foate's translation of Vesalius, and to further communications from the pen of Dr. J. K. Fotheringham, whose work peculiarly illustrates the aid rendered to modern scientific investigation by a wide and deep knowledge of ancient science. Less conspicuous but very real and useful will be the results of the tireless devotion which Dr. Withington has for many years bestowed on the vast mass of Greek scientific literature, with the object of rendering the forthcoming edition of *Liddell and Scott's Greek Lexicon* more complete in what has been hitherto a somewhat neglected department. It is a legitimate hope that his unique experience may now be utilized in rendering accessible to English readers the more important works of Galen.

Mrs. Singer's catalogue of early scientific MSS. in this country is now in a serviceable state and freely open to students. It is a card index with about forty thousand entries arranged by subject. The first section, Alchemy, is being printed as the opening fascicule of an international catalogue of Alchemical MSS., issued by the Union Académique Internationale under the general editorship of Professor Bidez of Ghent. The next of Mrs. Singer's sections

to appear will probably be 'Anatomy' and 'Aristotle' by the Editor of this series.

Of foreign workers in our field mention must be made in the first place of the veteran scholar Professor K. Sudhoff of Leipzig, who has for many years been producing volume after volume of highly original researches, covering wide stretches in the history of science. His extraordinary fertility and equally great literary generosity lay all other workers in his department under a debt of gratitude impossible to repay. It is gratifying to observe that his venerable colleagues, Professor Eilhard Wiedemann of Erlangen and Professor J. Hirschberg of Berlin, are still prosecuting their researches in Oriental Science and the History of Optics, and that fascicules of the *Corpus Medicorum Graecorum* and *Corpus Medicorum Latinorum* continue to appear, though all too slowly. The recent output by Professor Max Wellmann on the science of classical antiquity has been very remarkable. Another useful work in the department of the History of Science is Professor E. O. von Lippmann's *Entstehung und Ausbreitung der Alchemie.*

In Austria the activities of the most philosophical of living medical historians, Professor Neuburger, have continued in spite of the terrible conditions through which the city of Vienna has passed and is passing. The first volume of his illuminating *History of Medicine* was rendered into English by Dr. Ernest Playfair, with the encouragement of Sir William Osler. The appearance of the second volume of this translation was a cherished wish, as it would be a suitable memorial, of that lover of learning.

The recent output in the History of Science of some of the smaller European States has of late years been truly remarkable, and the world owes to them a number of works of primary value for the subject, elaborately published at vast expense. The decipherment, transcription, translation, and publication of the beautiful Windsor *Quaderni d'Anatomia* of Leonardo have now been completed by Professors Fonahn, Hopstock and Vangensten, and the work has been published at Christiania. The Société Hollandaise des Sciences is proceeding with the monumental

edition, collected from manuscript sources, of the *Œuvres complètes* of Christiaan Huygens. Denmark has given us the *Opera Philosophica* of Stensen, edited by Dr. Vilhelm Maar, the *Opera Omnia* of Tycho Brahe, edited by Dr. J. L. E. Dreyer, and the *Scientific Papers* of H. C. Örsted; all three are produced by the Carlsbergfond. Professor Heiberg of Copenhagen continues his series of fine works on Greek science, and his writings are an additional adornment of the Danish School. The Union Astronomique Internationale, founded at Brussels in 1919, has appointed a *Commission de réédition d'ouvrages anciens.*

In Italy the history of science has now been placed on a firm academic basis. The work of Professor Antonio Favaro on the physics and mathematics of the seventeenth century and of Professor Piero Giacosa on mediaeval science continues. The new *Archivio di Storia della Scienza* has completed its first annual volume under the editorship of Professor Aldo Mieli.

In France, since the appearance of Adam and Tannery's edition of the Works of Descartes and the termination by death of the labours of Henri Poincaré, the most important publication has probably been the fifth and posthumous volume of Pierre Duhem's very valuable *Le système du monde, histoire des doctrines cosmologiques de Platon à Copernic.* Mention must also be made of the very scholarly work on the *Commentaires de la Faculté de Médecine de l'Université de Paris* (1395–1516) by Dr. Ernest Wickersheimer, who has since become librarian of the University of Strassburg.

Nowhere, perhaps, has more general interest been taken in the History of Science than in the United States. Many courses of lectures have been devoted to the subject; much valuable original work in the department of mathematics has appeared from the pens of Professors L. C. Karpinski and Eugene Smith, in medicine from Colonel Garrison and Dr. E. C. Streeter, and in bibliography from Dr. A. C. Klebs. Among recent American publications are Mr. T. O. Wedel's work, *The mediaeval attitude toward astrology*, and a useful translation of Choulant's *History of Anatomic Illustration*, annotated and brought up to date by

Mortimer Frank, whose death at the age of forty-four is a real loss to learning.

Dr. George Sarton has been carrying on at Harvard the labours commenced before the war in Belgium. His journal, *Isis*, which first appeared in 1913 as a ' Revue consacrée à l'histoire et à l'organisation de la science et de la civilisation ', has revived since the peace, and some form of joint publication is projected between this series and *Isis*. Moreover, in the future, the *Studies in the History and Method of Science* will appear regularly as an annual volume.

A central institute and library, devoted to the promotion of systematic investigation into the historical documents of science, is greatly needed in this country. Such a foundation would do much to place the subject on its proper academic basis and would rapidly react on the whole system of scientific education. It would help the teacher to present the sciences in their evolutionary relation to each other and to the course of history as a whole. It would especially help the teacher of science to develop his subject as the product of a progressive revelation of the human spirit rather than as a mere description and attempted explanation of the phenomena. We may well look to this new orientation of scientific teaching to counteract the effects of the regrettable but real decline in the study of the older ' humanities '.

CHARLES SINGER.

University College, London,
*February, 1921.*

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# GREEK BIOLOGY AND ITS RELATION TO THE RISE OF MODERN BIOLOGY

By Charles Singer

*There is an extreme affecting of two extremities: the one antiquity, the other novelty; wherein it seemeth the children of time do take after the nature and malice of the father. For as he devoureth his children, so one of them seeketh to devour and suppress the other; while antiquity envieth there should be new additions, and novelty cannot be content to add but it must deface: surely the advice of the prophet is the true direction in this matter,* State super vias antiquas, et videte quaenam sit via recta et bona et ambulate in ea. *Antiquity deserveth that reverence, that men should make a stand thereupon and discover what is the best way; but when the discovery is well taken, then to make progression. And to speak truly,* Antiquitas saeculi iuventus mundi. *These times are the ancient times, when the world is ancient, and not those which we account ancient* ordine retrogrado, *by a computation backward from ourselves.*—Bacon's *Advancement of Learning*, v. 1.



## I. The Course of Ancient and of Modern Science compared

In the pages which follow we discuss certain elements in the exact, classified and consciously accumulated knowledge of living things possessed by the Greeks. This biological knowledge and the mode in which it was attained are well suited to the

illustration of Greek scientific method, for the actual achievements of the Greek mind were no less remarkable and perhaps more characteristic in Biology than in other departments of physical science. As a preliminary to the discussion we may briefly consider the means available for forming an estimate of Greek science as a whole, and in doing this we shall inevitably compare and contrast the science of antiquity with that of our own time.

Ever since man has been man, he has had some control over nature through his power to adapt his instruments to make her serve his will, and it is possible to define science in terms of this power and of the knowledge that lies at the back of it. But the *conscious* formulation of theories to explain natural phenomena, and the *conscious* collection and record of data as a basis of these theories, come as a far later phenomenon in human development. It is this conscious and more sophisticated process to which, for our present purpose, we shall give the title *science*, and science so interpreted cannot be traced with certainty earlier than the speculations of the Ionian philosophers of the sixth century B.C. Greek science thus established continued its course of positive achievement until the second or third century of the Christian era. Then, from causes which we need not here discuss, it ceased to be original, having run a course of some eight hundred years.

Our effective record begins with the *Hippocratic collection.* Some elements in this are at least as early as the sixth century B.C., and it is therefore impossible that these earliest portions should be the work of Hippocrates himself who died in the first half of the fourth century. Nevertheless Hippocrates is almost certainly the first scientific writer of whom we have substantial remains. The latest original Greek scientific works were perhaps those of Galen and Ptolemy of the end of the second century of the Christian era, or, if we should include mathematics in our scheme, we may carry the period forward to Diophantus of the third or even to Theon of Alexandria of the fourth century.[1]

We may compare this course with the science of our own time. For a thousand years and more after the downfall of Greek science, the powers of observation and the scientific imagination of mankind seemed to sleep, a sleep broken only by disorderly dreams which either fitfully recapitulated the past, or conjured up what never was and never will be. At length, however, Man awoke to

[1] Or perhaps to Theon's daughter Hypatia, who survived to the second decade of the fifth century and is said to have made original mathematical investigations.

look around and to examine the world into which he had been born. During the long twilight of a new dawn he had been stirring in his slumber, but the year 1543 gave full proof that the night was over and he was at last awake. In that year appeared the two works which mark the real sunrise of modern science, the *De revolutionibus orbium celestium* of the Pole, Nicolaus Copernicus, and the *De fabrica corporis humani* of the Belgian, Andreas Vesalius. These two were the first great modern natural historians of the Universe and of Man, of the Macrocosm and the Microcosm, and if any single year be selected, 1543 has perhaps a better claim than any other to be regarded as the birth year of modern science, though we shall see good reason for assigning its conception to a much earlier period.

Beginning from 1543 we are thus near the end of the fourth century of modern science, and are now at about the middle of the total period of time that Greek science had to run. During these four hundred years a vast and ever-growing mass of original investigation has been recorded, and as time has gone on the stream has grown ever broader and fuller. Some idea of its enormous and unreadable bulk may be gained by a glance at the *International Catalogue of Scientific Literature*[1] which, while giving the titles alone of original articles, consists each year of seventeen closely printed volumes. The vast intellectual effort which this enormous output implies has gradually transformed our mode of life, our attitude to the world around us, and even our hearts and minds.

Now if we seek to compare this extraordinary movement and its results with its prototype of antiquity, we encounter difficulties at the very outset. These difficulties of comparison lie not so much in the relative scantiness of the Greek record—that in itself might be an advantage for our purpose—but rather in the character of that record. The differences in the mode of recording ancient and modern science become explicable when we consider certain differences in the history of the two systems, and to their history we therefore turn.

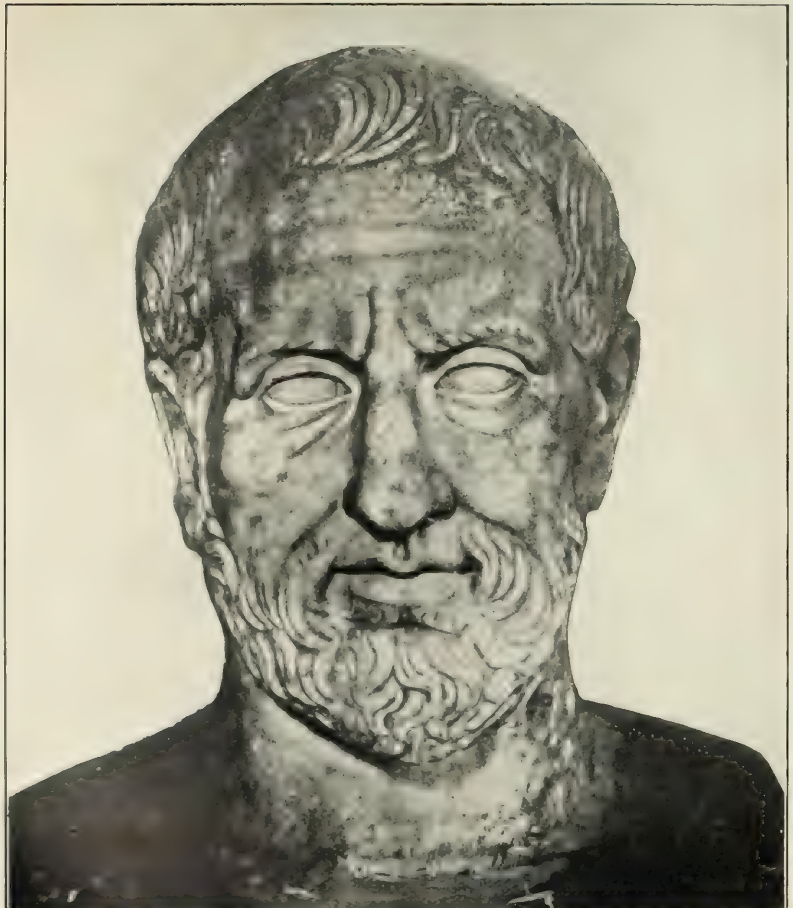
The earliest science, in the sense that we are using the word, arose in Asia Minor on the confines of the great Eastern

[1] Published for the International Council by the Royal Society of London. First annual issue 1902, last annual issue (with sequence disturbed by the intervention of the war) 1914–16. It is significant that the number of biological papers recorded in this enormous index is double that of the physical and mathematical combined.

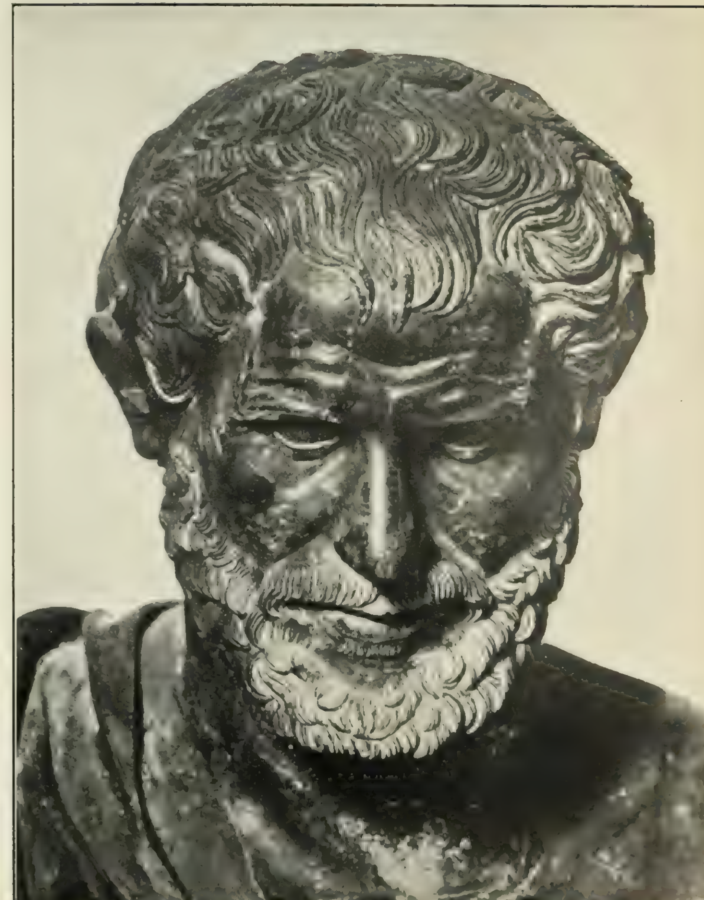
civilizations. In the social systems of the valleys of the Euphrates, Tigris, and Nile there had accumulated a great mass of observations, and upon them rough generalizations had been erected. These generalizations seem in the main to have been an evolutionary product of the 'social consciousness', rather than the definite fruit of individual minds, and it is thus characteristic of the science of the ancient East that its products are anonymous. From all the centuries of intellectual activity of the civilizations of Babylon and of Egypt, hardly even the name of a scientific discoverer has come down to us. It was into a great *impersonal* heritage that the philosophers of the Ionian cities were fortunate enough to enter ; with it as a basis they began to engage upon that active process of cosmical speculation that developed as Greek philosophy.

As time went on, knowledge accumulated, separate sciences or departments of knowledge were gradually differentiated and, in the course of centuries, these became more and more distinct from the parent stock of philosophy. Yet it is peculiar to Greek scientific thought that it never loses direct touch with the philosophic stem from which it sprang. Whether we look to such early traces of the scientific spirit as that of the sixth century B.C., when Pythagoras was contriving his first formulated conceptions of the relations of number to form, or whether we consider the last vitally original works of Greek science in the second century of the Christian era, when Galen and Ptolemy were giving forth those ideas on the structure of man and of the world that were to dominate Western thought for a millennium and a half, from end to end Greek science constantly betrays its descent from Greek philosophy.

Far different is the ancestry of modern science. The origin of modern science will be sought in vain in the lucubrations of the philosophers, who played but a subordinate part in the revival of letters. Copernicus and Vesalius were dead before the great philosophers of modern science, Francis Bacon and René Descartes, had been born. Nor is it a more fruitful task to attempt, as many have done, to draw a picture of our scientific system as but a rebirth of the wisdom of ancient Greece, for we must then seek its origin in the writings of the men who were the agents of that rebirth. Yet from them we get but little light. Science, as we understand the term to-day, was far from the minds of the men who made the New Learning. The scholars of the fourteenth and fifteenth centuries showed scant sympathy for the investigation of Nature



From VILLA ALBANI
Copy (IInd cent. A.D.?) of earlier work



From HERCULANEUM
Probably work of IVth cent. B.C.

PLATE III

LATE MINOAN GOLD CUPS. ATHENS MUSEUM

From VAPHIO about XVIth cent. B.C.

and the humanistic period dominated by them was, on the whole, backward or at best but retrospective in its scientific conceptions. Their thoughts were rather with the great past of literature and of art, which they sought to bring back to life.

It is certainly true that there were a few philosophical writers of the later part of this period in whom can be traced some consciousness of the value of the experimental method: Nicholas of Cusa (1401–64), Pomponazzi (1462–1525), Fracastor (1478?–1553) stand here to witness. But it is far from clear that their ideas on the mode of extension of natural knowledge were related to the re-discovery of the Greek texts and the diffusion of knowledge of the Greek language. These men, at best, were few and exceptional, and they come mainly in the late and academic period of the learned revival; their place is rather among the founders of modern science and they do not naturally fall into the series of the scholars of the classical Renaissance. It may, indeed, be claimed that the astronomical work of Regiomontanus (1436–76) and Purbach (1423–61) was dependent on their salvage of the text of Ptolemy.[1] But their recovery of the Greek original was the *result* of their scientific zeal and an incident in their scientific researches; it did not provide the primary stimulus for those researches.

If we turn to the revival of biological studies, we are encountered with the same phenomenon of dissociation from the learned revival of the Greek texts. The first modern records of the close scientific observation of plants or animals impinge on the intellectual orbit of the age either too early or too late to be explained as attracted thither by the new learning. Effective advance in zoological knowledge hardly begins until the second half of the sixteenth century, but it was preceded by the work of the anatomists whose activities we may trace back to the eleventh century. The records of botanical observation tell much the same tale. The familiar attempts of the 'fathers of Botany' are not encountered until well into the sixteenth century, but behind their work we can discern a long and slow evolution of first-hand plant-study reaching back to the twelfth century. However true it may be

[1] Purbach died before his project of obtaining the Greek text was attained. His *Theoricae novae planetarum*, Nuremberg, 1472, was published by Regiomontanus and relies on the text derived from Arabic. The *Epytoma Ioannis de monte regis* [i.e. Regiomontanus] *in Almagestum Ptolomei*, Venice, 1496, goes back however to the Greek.

that Greek thought is the final motive of these developments, that the desire to know is but the stirring again of the Greek spirit crushed and overlaid by barbarism and misunderstanding, it is yet clear that the actual recovery of the texts had no very close relation to the recommencement of direct biological observation. Above all, we need to distinguish mere *passive increase* of knowledge brought by the revival of the Greek language from the *active extension* of knowledge by direct observation that is the essence of the experimental method. This process of active extension began centuries before the learned Greek revival and received its great impetus long after it.

Thus modern science arose largely independent both of philosophy and of scholarship. She was, in truth, conceived in obscure and humble circumstances before the days of the new learning and in very different surroundings from those of her older sister. She did not take her origin, as did Greek science of old, among a group of philosophers thinking at large, and with little to check their investigations and their speculations save the limits of their own intellectual powers and a slowly accumulating mass of observations. The lines of modern physical science fell in far less pleasant places. Modern science entered on her first period of development in a highly sophisticated society, ruled intellectually by a most rigid tradition, limited by the claims of a priesthood, and constantly checked by an interpretation of Scripture which was only one degree more fettering than the scholastic presentation of such Greek philosophy as had reached that age. It was in the twelfth or perhaps the early part of the following century, with the flowing tide of the scholastic movement, that men began consciously but very slowly to modify by observation the details of a vast tradition concerning the structure of the universe and of man.[1]

[1] Some reservation, so far as Anatomy is concerned, must be made for the School of Salerno, where we have evidence that animals were being dissected at the end of the eleventh century. There is, however, no evidence that this example was followed elsewhere for generations, and perhaps the work of Salerno, so far as it is really scientific, is more logically regarded rather as the last phase of the ancient than as the first phase of modern science. The two earliest Salernitan anatomies are of the pig and they date respectively from about 1085 and 1100; they may be most conveniently consulted in Salvatore De Renzi, *Collectio Salernitana*, 5 vols., Naples, 1852–9, vol. ii, pp. 388 and 391. Even the earlier of these two tracts shows some trace of Arabian influence. Light is thrown on these early anatomical tractates by a recent excellent graduation thesis of a pupil of K. Sudhoff, F. Redeker, *Die 'Anatomia magistri Nicolai phisici' und ihr Verhältnis zur Anatomia Chophonis und Richardi*, Leipzig, 1917.

PLATE IV Cotton Vitellius C. III fo. 19r. Anglo-Saxon Work c. 1000


PLATO & CHIRON HAND MEDICAL ART TO AESCULAPIUS
Contrast typical Early English drawing of animals and plants with Classical work of frontispiece and Renaissance work of Plate XI

The process of development was perforce slow, for in the social and intellectual environment of the age speculation was impossible, and the only practicable advance was that of repeatedly verified experience. It was easy to confine a philosopher to his monastic cell for alleging aught new of the constitution of matter or of the structure of the heavens, but less easy, even in those days, to deny that his gunpowder exploded, when it did explode, or that his glasses magnified, when they did magnify. Princes and prelates had no need of the aid of Greek thought to discern the advantages that they might derive from the applications of gunpowder, nor did they await the re-discovery of Greek letters to become sensible of the uses of spectacles. If necessity is the mother of invention, experience is her father, and these two rather than Greek letters and Greek philosophy were the real begetters of the new experimental method.[1]

Perhaps it is in the order of nature that the root of the tree of knowledge is bitter though its fruit be sweet; it is at least true that the lesson of scientific verification needed to be well learnt before scientific speculation and theorization could become profitable. That lesson had at last been learnt, and we may be assured that the profound difference in the manner of setting forth the science of the modern and that of the ancient world is an expression of the historic difference in the origin of the two systems.

## II. The Record of Ancient and the Record of Modern Biology

Consider examples of the two methods concretely and, turning to one of the thousands of files of journals that make up the bulk of any modern scientific library, compare its contents with one of the scientific works of Aristotle or of Theophrastus. Since we are discussing Biology, we will select a biological journal and briefly examine an average article in it, for such articles on special and narrow problems are a peculiar product of modern science and in these 'memoirs' modern science is most characteristically represented.

[1] In the history of scientific development mathematics and the mathematical sciences stand somewhat apart from the other departments of knowledge. It is a point that cannot be treated here, but it has been briefly discussed by the present writer in a pamphlet, *Greek Science and Modern Science, a Comparison and a Contrast*, London, 1920.

The author begins by pointing out a gap in knowledge. The structure or habits of some rare organism, he tells us, are inadequately known, or the development of some plant may be expected to throw light on the relationship between two groups of plants, or perhaps the function of an organ requires further elucidation. Having stated his problem, he reviews the efforts made by others to illumine this dark place in knowledge. He points out some of their errors or decides to accept their work and base his own upon it. Perhaps he distrusts their experiments or would like to re-interpret their results. Having summarized their labours he details his own experiments and observations.

But he is not able to tell us all of these. If he did, scientific literature would be far more bulky than it already is and science would quickly perish, suffocated under the dead weight of its own verbosity. Our author must, in fact, omit a great many of his mental processes. Space will not permit him to tell us how he embarked on many different lines of work and abandoned them as unprofitable or too difficult, nor anything of the months or years spent in merely repeating the experience of others. He says no word of how he acquired and improved his experimental skill and technical experience. He tells merely of those developments of his work that have yielded him results. But he does not tell us all even of these. When by gradual steps he had at last reached, or perhaps with the instinct of genius had more quickly discerned, a profitable direction for his investigations, he reached after a time those conclusions which his final line of work has verified and rendered more exact. It is this final process of verification that he mainly describes in his article, and it is the details of this that occupy the bulk, perhaps nineteen-twentieths or more, of all that he has to say. Then having described these verificatory experiments, he summarizes his conclusions in a short paragraph of a few lines.

That is a fair description of the average piece of scientific work as it is turned out to-day, and from vast accumulations of such work the generalizations of men of scientific genius have been mainly though sometimes unconsciously built up. The mass of scientific writings, bulky as they are, contain descriptions of only the verificatory experiments, and it is on account of this necessary limitation that it is impossible to understand scientific method from books without making independent observations.

How does the science of antiquity compare with material such as this? The Greek work is of course less in quantity and often

OROBUS SP.

From MS. Bodley 130 fo. 16r

Written 1120

BETONY

From MS. Ashmole 1462 fo. 12r


Early XIIIth Cent.

TEUCRIUM CHAMÆDRYS

From MS. Bodley 130 fo. 16r

Written at St. Albans 1120

PLATE V

fragmentary in character, yet it is not that which makes comparison difficult. The difficulty arises from the habit of the Greek writers of setting down only their conclusions. Their methods of work, even their verificatory observations, they have almost completely hidden from us. It is as though we had a collection of the last few lines of a series of scientific articles. To grasp the nature of modern scientific method from a scientific article is difficult, since not all the mental processes involved are represented. In the case of Greek science the difficulty is far greater, for here we have only the conclusions with hardly any of the processes. There survives to this day a form of scientific literature, or rather of literature on scientific topics, which bears a distant analogy to some parts of the Greek record as it has reached us, though its motive is utterly different. It is the type of work known as the 'cram-book', which merely summarizes the known or recognized facts without reference to sources or methods. Such works present us with the final results of vast amounts of intellectual effort but they give little information as to how those results have been obtained.

Now it would appear that it is not the accidents of time that have reduced the Greek record to its present state. The responsibility rests far more with the Greeks themselves and with their method of research. The Greek scientific work lacks nothing in brilliance, the Greek scientist yields to none in keenness, the Greek record is at least the equal of our own in clearness. It is the constant solicitude for the exact *mode* of investigation, a solicitude characteristic of our own science, that we so often seek in vain among the Greeks. The method of modern scientific research is before all things and above all things a process of verification. 'The complexity of phenomena', says a critic of Aristotle, 'is as that of a labyrinth, and one wrong turn may cause the wanderer infinite perplexity. Verification is the Ariadne-thread by which alone the real issue must be sought.'[1] Yet the process of verification is slow and tedious and often difficult and dull, and Man is by nature lazy and impatient, hating labour yet eager for results. Hence Credulity. Credulity is the Pandora of Science, promising everything, yielding nothing. 'If you trust before you try, you may repent before you die' is as good a maxim in scientific as in human relationships. Μέμνησο ἀπιστεῖν would have been a salutary phylactery for the Greek scientific Pharisee, serene in

[1] G. H. Lewes, *Aristotle: a chapter from the history of science*, London, 1864.

the conviction of the power of his reason, to have bound upon his hand and to have worn as a frontlet between his eyes. It is true that the great Greek minds were singularly free from that baser credulity that we call superstition—from that they were preserved by their conviction that order reigned in Nature—but few indeed were the Greeks who showed an adequate scientific scepticism. The Greek often accepted data without scrutiny, induction without proof. His very brilliance was a source of weakness and he was often led to believe that the order of phenomena must perforce correspond to his own admirably clear conceptions.

But although Greek science failed to lay the enormous stress on verification that we in this last age have found to be the chief condition of scientific progress, it is yet certainly true that all Greek scientists were not equally deficient in this respect. Aristarchus of Samos, Archimedes of Syracuse, Hero of Alexandria, these are illustrious exceptions, and the lasting value of their results remains the justification of their method. Further, the process of verification is easier in some sciences than in others. Such are those purely observational studies classed under the general title of Natural History, for in them the phenomena are visible to all and the difficulty usually lies rather in the collection and arrangement of the vastly numerous data than in their verification. Moreover, biological phenomena are so exceedingly complex that it is very difficult to form general theories to explain any considerable proportion of them. The Greek naturalist was thus less tempted than the Greek physicist or astronomer to fit his facts to his preconceived theories.

Yet even in the field of natural history the Greek character had its own special sources of weakness. The Greeks were gifted with such a variety of talents that there may be a tendency to credit them with qualities that were not theirs, for although extraordinarily anxious to explain natural phenomena they were not remarkably observant of Nature except when their attention was specially drawn to it. It was the world *in relation to himself* and not as a mere objective complex of phenomena that interested and appealed to the Greek. Thus in his attention to purely external phenomena he ranks no higher than many peoples infinitely his inferiors in other respects. It is said that any Malay can distinguish some three hundred species of insects ; set off against this the whole fauna of Aristotle with its total of but five hundred and forty forms. Again, Theophrastus, a professed botanist, mentions only

three hundred plants, practically all cultivated; compare this to the record of barbarian Anglo-Saxon speaking tribes of whose literature the merest fragment survives, but a fragment containing about eight hundred plant names.[1]

But when the attention of the Greek was once fixed upon the structure or habits of living things, his success in elucidating or portraying them was unrivalled, for then the living things became part of his own world and not merely of the world around him, personal and not impersonal. With us it is quite otherwise, for it is just the impersonal or objective study that gives the hall-mark to our science and perhaps also to our art, and it is exactly in this objective or impersonal aspect that the Greek often failed. And he well knew his own weakness, which Plato has exposed for us with a sure hand: 'If we consider', he says, 'the works of the painter and the different degrees of gratification with which the eye of the spectator receives them, we shall see that we are satisfied with the artist who is able *in any degree* to imitate the earth and its mountains, and the rivers, and the woods, and the universe, and the things that are and move therein; and further, that knowing nothing precise about such matters, we do not examine or analyse the painting; all that is required is a sort of indistinct and deceptive mode of shadowing them forth. But when he paints the human form we are quick at finding out defects, and our familiar knowledge makes us severe judges of one who does not render every point of similarity.'[2] This criticism of Plato is well borne out by a study of Greek art.

If we examine the attempts to represent the forms of the animal and vegetable creations by early peoples, a very striking feature presents itself. A large number of ancient delineations of animals and plants have come down to us, and these figures show that the habits and forms of moving creatures fixed the attention of almost all races long before the same care was expended on plants. Animals are more like ourselves than plants, they move and feel and are subject to pleasure and passion, and so are capable of more personal treatment. The natural interest in the animal rather than in the plant might be illustrated by a hundred instances from the palaeolithic cave paintings downwards, but

[1] It is true that of these Early English plant names many are derived from Greek (through Latin) and many have only a literary use. But even allowing for these tendencies their number remains remarkable.

[2] *Critias.*

we will take our example from a pre-Hellenic people in the land of the Greeks. The best known of all the Minoan relics are perhaps the Vaphio cups (Plate III), and these betray the most careful study of the structure and movements of the bull. His anatomy is accurately shown and we can clearly discern the surface markings raised by the muscles which move the shoulders and the hind-quarters, as well as by those which support the head and control the ribs. Yet the representations of plants on these cups are very poor, so that the trees cannot be identified.

This neglect of plants, the beings least like ourselves, is characteristic also of the Hellenic art that succeeded and replaced the Minoan, and it has its analogue in Greek science. In the Greek scientific writings the interest in plants is usually practical; even Theophrastus, dealing almost exclusively with the domesticated varieties, frankly tells us that it was their medical application that had led to their more accurate study; the same is true of Dioscorides and of a number of minor Greek authors who have written on plants and their properties. We may search classical art in vain for any striking figures of plants, and the best representations come to us from a time when the creative force of Greece was dead. The most accurate Greek representations of plants are in a series prepared by artists of Constantinople as late as the sixth century of the Christian era (Plates VI and VII). The draftsmen of the Julia Anicia MS. of about 512 represent their originals faithfully and accurately, point by point, almost hair by hair, but with no trace of imaginative treatment.[1] These degenerate scions of the mighty race of Pheidias and Apelles were producing pictures of plants which are indeed no works of art but are yet accurate and clear and represent their subjects much as the illustrator of a modern scientific treatise might seek to do.

If we turn from the graphic representation of living things to scientific discourse about them we find ourselves in face of an extensive literature. Conrad Gesner, the most learned of biologists, estimated the number of Greek works with considerable bearing on Botany as over a hundred, and although many of these, it must be admitted, are very trivial, yet about half of them

[1] It is true that some of the figures in this MS. and possibly all of them are copied from earlier MSS. and not directly from nature. But even the originals, as shown below (p. 63, fig, 29), must have been long posterior to the best period of Hellenic, though contemporary with the so-called Augustan art with its remarkable treatment of plant forms.

PLATE VI

Julia Anicia MS. fo. 315 r Vth-VIth cent.

ΣΟΓΚΟΣ ΤΡΑΧΥΣ SOW-THISTLE

. PLATE VII

Julia Anicia MS. fo. 370 v Vth–VIth cent.

ΦΑϹΙΟΛΟϹ SEEDLING BEAN

still find a place in the most exhaustive modern history of botany.[1] The number of works on the structure and habits of animals is also considerable. Now if the Greek was interested in men rather than things, as Plato tells us, how account for all this output? What is it that thus fixed the attention of the Greek on animals and plants? The answer is that these works have in the main a practical end; the plants and animals are described for the use to which they can be put by man. But there remains a small residuum of works, chiefly those of the Lyceum, which have no such end in view. Why were these written, and where among the self-centred Greek people was the public interested in natural knowledge with no direct application to the circumstances of life? The answer is that the best Greek biological opinion had come to regard Man himself as a natural product and was growing accustomed to look upon him as a member of a whole series of beings. These beings extended to the supra-mundane spheres, but the lower series also, plants and animals, partook of his essence in varying degrees, their resemblance to him increasing with their higher rank in the scale. Thus animals and plants, but especially animals, helped Man to understand *himself*.

## III. The Bases of the Aristotelian Biological System

### (*a*) *Classification*

Of the biological researches of the Lyceum we have the three great Aristotelian works, the *Historia animalium*, the *De partibus animalium*, and the *De generatione animalium*, and on plants the *Historia plantarum* and the *De causis plantarum* of Theophrastus, the pupil and successor of Aristotle, as well as the later and imperfect peripatetic work *De plantis*, probably composed by Nicholas of Damascus in the first century B.C.[2] There are also

[1] E. H. F. Meyer, *Geschichte der Botanik*, 4 vols., Königsberg, 1854–7.

[2] The history of this work is curious. The original work on plants by Aristotle was commented on by Nicholas in Greek. This commentary was translated by Hunein ben Ishâk into Syriac, and this translation was turned, by his son, into Arabic. In its Arabic dress it was then modified by Thâbit ben Curra. From the Arabic it was twice translated into Latin in the thirteenth century, on one occasion by the shadowy and elusive Alfredus Anglicus. An authoritative edition of the Latin text of Alfredus was published by E. H. F. Meyer, *Nicolai Damasceni de plantis libri duo Aristoteli vulgo adscripti*, Leipzig, 1841. See especially F. Wüstenfeld, *Die Uebersetzungen arabischer Werke in das Lateinische seit dem XI. Jahrhundert*, Göttingen, 1877.

two Aristotelian works on the movements of animals with which we are not here concerned, nor shall we take into consideration such points in Aristotle's works as deal with the structure of man, since these are best reserved for separate treatment. The Aristotelian and zoological writings may be considered first.

Whatever be concluded as to other works of Aristotle, it is probably true that any biologist who examines his zoological writings will accept what is known as the 'note-book theory'. The ill arrangement of much of the material and the gravity of some of the errors make it difficult to conceive that these works are in the state designed for publication by the master, with his genius for classification and undeniable powers of observation. The only explanation that will satisfy is that the more serious blunders are the mistakes of the student note-taker who had in his hands the rough-note books of the teacher. It is therefore probably true that if Aristotle's best biological observations are taken as samples of his scientific work we shall obtain the truest picture of what he himself was accustomed to teach.

There can be no doubt that through much of the Aristotelian writing there breathes a belief in a *kinetic* as distinct from a *static* view of existence. It is this aspect of his teaching which brings all living things into relation with man. In Aristotle's study of animal forms there are two departments where this kinetic view gains specially clear expression. These departments are respectively the Arrangement or Classification of Animals, and their Development, or, as we now call it, their Embryology.

It is now customary to summarize our knowledge of living beings in tabular form. As interpreted by a modern biologist these classificatory tables represent certain degrees of relationship. Modern systems of classification are not, however, as is often thought, closely comparable to genealogical trees, because two species may be very nearly allied and therefore close together in the table of classification, although they parted company far back in their history, or again historically allied species may become widely differentiated in comparatively few generations. Classificatory tables are rather intended to summarize structural similarities and structural differences, though, as a rule, the naturalists who draw them up have no exact quantitative conception of the amounts of differences signified by the degree of separation in their tables. Indeed this absence of a quantitative factor is among the weakest points of modern biology, and it is

only in very recent times that effective attempts have been made to remedy it.

Now despite the fact that no classificatory table of Aristotle has come down to us, there can yet be no doubt that he formed general ideas of a classification based on the consideration of the structure and habits of animals. He uses the terms of classification and speaks of larger and smaller groups of animals which bear greater or less similarity to each other. It is thus in accordance with his meaning and is perhaps reproducing his method of teaching if we draw up a classification of animals from his works.

The primary basis of his classification would surely have been the method of reproduction, a subject to which he paid a vast amount of attention (cf. Table on p. 16). It is therefore necessary to examine some of his ideas on this subject.

He knew nothing of the mammalian ovum, and he regarded the mammalian embryo as a thing living from the first, and living in a higher sense than an egg can be said to live. The mammalia were thus for him viviparous internally and not merely externally in the sense in which the word viviparous is now used. The remaining *enaima*, sanguineous animals or ‘ vertebrates ’ as we now call them, were primarily oviparous, though some among them were viviparous in the external sense; that is to say, while the young in these cases were held to develop always from an egg, that egg might sometimes be hatched within the mother's body. The ‘ invertebrates ’, or *anaima* in Aristotle's notation, had on the other hand their own special methods of reproduction, among which the so-called *spontaneous generation* played an important part.

In considering the table of classification that we have drawn up from Aristotle's works and in comparing it with any modern system, the difficulties under which he was working must be recalled. He makes no attempt to produce a complete system, and he deals almost entirely with local forms. He exhibits knowledge of about 540 species of animals. When we consider that of insect species alone some half million are now recognized, a thousand times as many as his total fauna, we shall be more disposed to wonder that he has fastened upon so many points still regarded as of classificatory value, than to criticize his errors or the gaps in his knowledge.

ENAIMA. (Sanguineous and either viviparous or oviparous) = Vertebrates.

- Viviparous (in the *internal* sense)
  1. Man.
  2. Cetacea.
  3. Viviparous Quadrupeds :
     - (*a*) Non-amphodonta (Ruminants with cloven hoofs and incisors in lower jaw only).
     - (*b*) Monycha (with single hoofs).
     - (*c*) Other viviparous quadrupeds.
- Oviparous (though sometimes *externally* viviparous)
  - With perfect ovum
    4. Birds :
       - (*a*) Gampsonycha (Raptores with talons).
       - (*b*) Steganopodes (Natatores with webbed feet).
       - (*c*) Peristeroeide (Columbidae).
       - (*d*) Apodes (Swifts, martins, swallows).
       - (*e*) Other birds.
    5. Oviparous Quadrupeds (Amphibia and most Reptiles).
    6. Ophiode (Serpents).
  - With imperfect ovum
    7. Fishes :
       - (*a*) Bony fish.
       - (*b*) Selachia (Cartilaginous fish and Fishing-frog).

ANAIMA (Non-sanguineous and either oviparous, vermiparous or budding) =Invertebrates.

- With imperfect ovum . . .
  1. Malacia (Cephalopods).
  2. Malacostraca (Crustacea).
- With scolex . . . .
  3. Entoma (Insects, spiders, scorpions, &c.).
- With generative slime, buds, or spontaneous generation
  4. Ostracoderma (Molluscs except Cephalopods, Echinoderms, &c.).
- With spontaneous generation only . . . .
  5. Zoophyta (Sponges, Coelenterates, &c.).

Aristotle's primary division into Enaima and Anaima, or as we call them Vertebrates and Invertebrates, is one still universally accepted. The two groups are now, it is true, regarded as incommensurate in evolutionary value, but this has only been recognized during the last generation or two, and the division survives as an effective dichotomy of our knowledge. When we examine his Enaima we see a division into groups which, with the forms known to him, could hardly be improved. The fish-like Cetacea are separated too widely from the other mammals, but Aristotle nevertheless knows of their mammalian character and recalls the fact that they suckle their young. He is in no danger of confusing them with fish. 'All animals', he says, 'that are internally and externally viviparous have breasts, as for instance all animals that have hair, as man and the horse, and the cetaceans, as the dolphin, the porpoise, and the whale, for these animals have breasts and are supplied with milk. Animals that are oviparous

PLATE VIII. Leyden MS. Voss Lat. Q. 9 Apuleius VIth cent.


fo. 83 r *Fraga* = Blackberry

or only externally viviparous have neither breasts nor milk, as the fishes and the bird.' [1]

The passages in which Aristotle describes the Cetaceans are worth putting together as giving a general idea of his powers of zoological classification, and they provide an excellent illustration of the stage that he had reached in the study of comparative anatomy and physiology.

'Some animals are viviparous, others oviparous, others vermiparous. Among the viviparous are man, the horse, the seal, and all other animals that are hair-coated, and, of marine animals, the cetaceans, as the dolphin and the so-called Selachia.[2] . . . The dolphin, the whale, and all the rest of the Cetacea, all, that is to say, that are provided with a blow-hole instead of gills, are [internally] viviparous. That is to say, no one of all these is ever seen to be supplied with eggs, but directly with an embryo from whose differentiation the animal develops, just as in the case of mankind and the viviparous quadrupeds.[3] . . . All creatures that have a blow-hole respire and inspire, for they are provided with lungs. The dolphin has been seen asleep with his nose above water, and when asleep he snores.[4] . . . One can hardly allow that such an animal is terrestrial and terrestrial only, or aquatic and aquatic only, if by terrestrial we mean an animal that inhales air, and if by aquatic we mean an animal that takes in water. For the fact is the dolphin performs both these processes: he takes in water and discharges it by his blow-hole, and he also inhales air into his lungs; for the creature is furnished with these organs and respires thereby, and accordingly, when caught in the nets, he is quickly suffocated for lack of air. He can also live for a considerable while out of the water, but all this while he keeps up a dull moaning sound corresponding to the noise made by air-breathing animals in general; furthermore, when sleeping, the animal keeps his nose above water, and he does so that he may breathe the air. . . . For the fact is, some aquatic animals [as fish] take in water and discharge it again, for the same reason that leads air-breathing animals to inhale air; in other words, with the object of cooling the blood. Others [as cetaceans] take in water as incidental to their mode of feeding; for as they get their food in the water they cannot but take in water along with their food.' [5]

'The dolphin bears one at a time generally, but occasionally two. The whale bears one or at the most two, generally two. The porpoise in this respect resembles the dolphin. . . . The dolphin and the porpoise are provided with milk, and suckle

[1] *Historia animalium*, iii. 20; 521[b] 21.
[2] *Historia animalium*, i. 5; 489[a] 34.
[3] *Historia animalium*, vi. 12; 566[b] 2.
[4] *Historia animalium*, vi. 12; 566[b] 12.
[5] *Historia animalium*, viii. 2; 589[a] 31.

their young. They also take their young, when small, inside them. The young of the dolphin grows rapidly, being full-grown at ten years of age. Its period of gestation is ten months. It brings forth its young in summer, and never at any other season. Its young accompany it for a considerable period; and, in fact, the creature is remarkable for the strength of its parental affection.'[1]

The *Historia animalium* in which these passages occur became accessible in versions by Michael Scot (1175 ?–1294 ?),[2] by Albertus

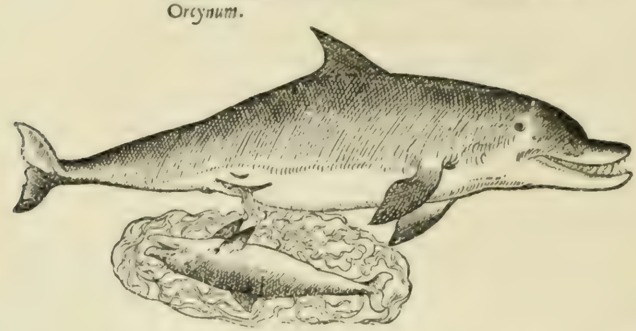



FIG. 1. GRAMPUS AND NEWLY-BORN YOUNG

The foetus is still surrounded by its membranes and the after-birth is in process of extrusion. From Pierre Belon, *Histoire naturelle des estranges poissons marins, avec la vraie peincture et description du daulphin et de plusieurs autres de son espèce*, Paris, 1551.

Magnus (1206–80),[3] and perhaps by William of Moerbeke (died *c.* 1281). The work was again rendered into Latin by Theodore Gaza, about 1450.[4] Yet the mammalian affinities of the Cetacea appear to have been generally overlooked in the Middle Ages and at the Revival of Learning until Pierre Belon (1507–64) repeated Aristotle's observations in the middle of the sixteenth century and published a description of the cetacean placenta

[1] *Historia animalium*, vi. 12; 566$^{b}$ 7.

[2] Translated from the Arabic. Cf. F. Wüstenfeld, loc. cit., p. 101. Roger Bacon tells us that in 1230 'Michael Scot appeared [at Oxford] bringing with him the works of Aristotle on natural history and mathematics, with wise expositors, so that the philosophy of Aristotle was magnified among the Latins'. It appears that Scot produced two versions of the *De Animalibus*, one entitled *De Animalibus ad Caesarem* and the other *Tractatus Avicennae de Animalibus*. He also incorporated ideas from the *De Generatione Animalium* in his *Liber de secretis naturae*.

[3] An edition of Albert's commentary has been produced by H. Stadler, *Albertus Magnus de animalibus libri XXVI nach der Cölner Urschrift*, Münster i. W., 1916 and 1921, in Baeumke's 'Beiträge zur Geschichte der Philosophie des Mittelalters.'

[4] First printed Venice, 1476.

(Figs. 1 and 2). Belon is further worthy of commemoration as he was perhaps the first among the moderns to make an attempt at a *comparative* anatomy, for in one of his works he sets forth the homologues of the vertebrate skeleton along somewhat similar lines to those of Aristotle[1] (Fig. 3), a conception soon developed by Coiter well beyond the Aristotelian level.[2]

The classification of birds is to this day in an unstable state. We may say that Aristotle's grouping is substantially that which prevailed in scientific works till recent times and still remains as the popular division. His separation of the cartilaginous from the bony fishes, on the other hand, still stands in scientific works, and is a stroke of genius which must have been reached by means of careful dissection. It is marred only by the inclusion of one peculiar bony form, the fishing-frog, or *Lophius*, among the cartilaginous fishes, and investigation shows that the skeleton of this creature is, in fact, peculiarly cartilaginous. Aristotle himself regarded the Lophius as aberrant among cartilaginous fishes.


FIG. 2. THE UTERUS OF THE PORPOISE

Opened to show foetus attached by umbilical cord to placenta. From Pierre Belon, *Histoire naturelle des estranges poissons marins, avec la vraie peincture et description du daulphin et de plusieurs autres de son espèce*, Paris, 1551.

For the Anaima or Invertebrates even modern systems of classification are but tentative. There is an enormous number of species, and after centuries of research naturalists still find vast gaps even in the field of mere naked-eye observation. Nevertheless, with the instinct of genius, and with only some 240 of these forms on which to work, Aristotle has fastened on some of the most salient points. Especially brilliant is his treatment of the Molluscs. There can be no doubt that he dissected the bodies and carefully watched the habits of octopuses and squids, *Malacia* as he calls them. He separates them too far from the other Molluscs,

[1] The suggestion had already been made, though in a less complete form, by Vesalius in the *De fabrica corporis humani*, 1543. There are also traces of the conception of a comparative anatomy in the MSS. of Leonardo da Vinci.

[2] Volcher Coiter, *Lectiones Gabrielis Fallopii de partibus similaribus humani corporis ex diversis exemplaribus*, Nuremberg, 1575.

grouped by him as *Ostracoderma*, but his actual descriptions of the structure of the Cephalopods are exceedingly remarkable. (Cf. p. 39 ff.) His distinction between the *Malacostraca* or Crustacea,

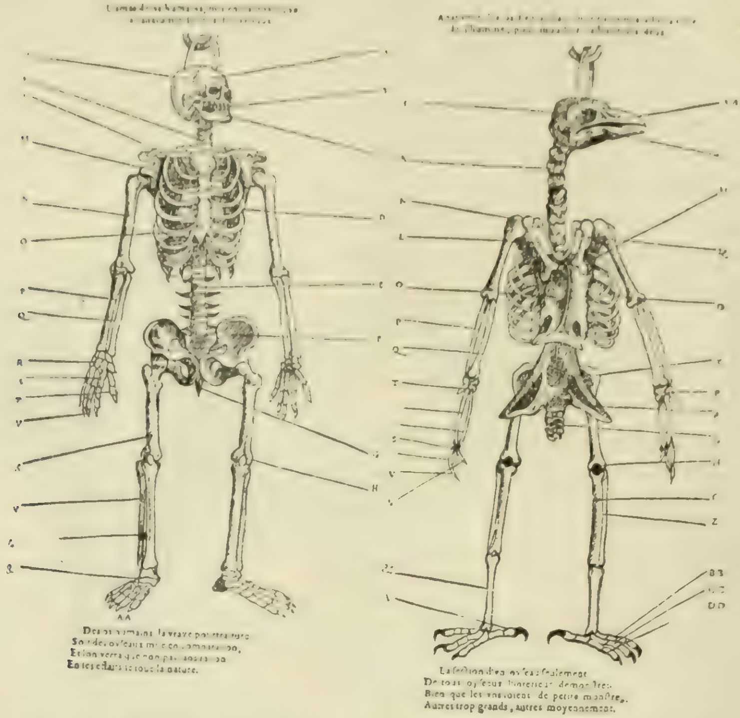


FIG. 3. THE SKELETON OF A MAN AND OF A BIRD COMPARED
From Pierre Belon, *L'histoire de la nature des oyseaux*, Paris, 1555.

*Entoma*, Sponges, and Jellyfish are also still of value, and these divisions remain along much the same lines as he left them.

### (*b*) *Phylogeny*

Aristotle nowhere formally exhibits either a ' Scala Naturae ' or a ' genealogical tree ', devices in which naturalists have delighted for the last two centuries, but he constantly comes so near to such conceptions that there is no great difficulty in reconstructing his scale from his descriptions (Fig. 4).

' Nature ', he says, ' proceeds little by little from things lifeless to animal life in such a way that it is impossible to determine the exact line of demarcation, nor on which side thereof an intermediate form should lie. Thus, next after lifeless things in the upward scale comes the plant, and of plants one will differ from another as to its amount of apparent vitality ; and, in a word, the whole genus of plants, whilst it is devoid of life as compared

with an animal, is endowed with life as compared with other corporeal entities. Indeed, there is observed in plants a continuous scale of ascent towards the animal. So, in the sea, there are certain objects concerning which one would be at a loss to determine whether they be animal or vegetable. For instance, certain of these objects are fairly rooted, and in several cases perish if detached.' [1]

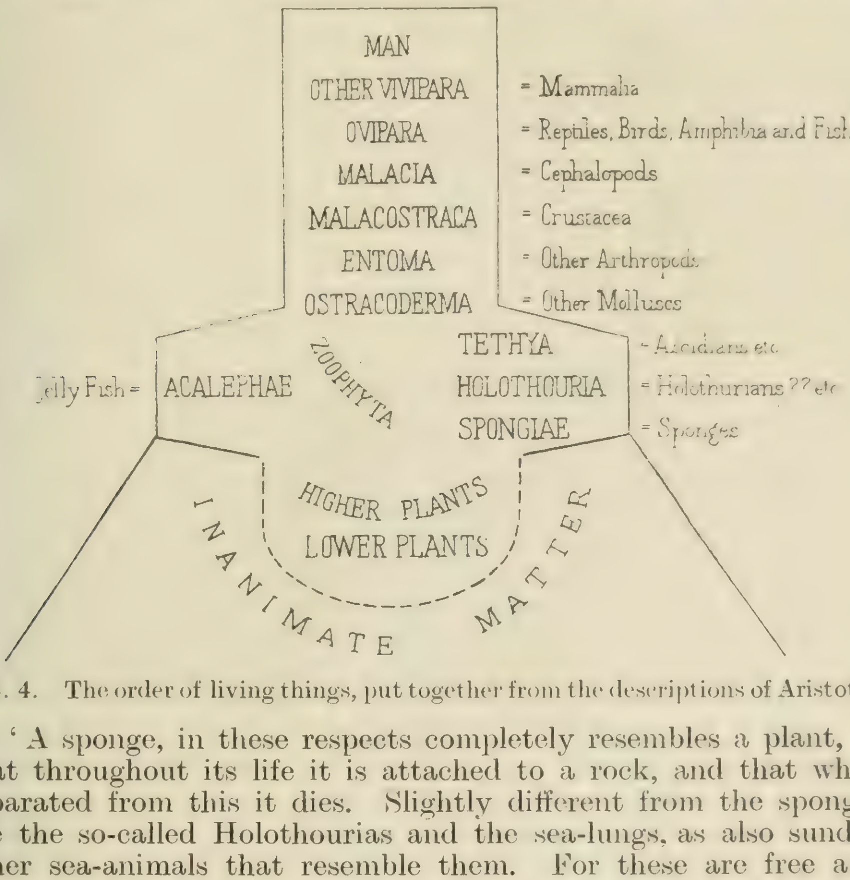



FIG. 4. The order of living things, put together from the descriptions of Aristotle.

'A sponge, in these respects completely resembles a plant, in that throughout its life it is attached to a rock, and that when separated from this it dies. Slightly different from the sponges are the so-called Holothourias and the sea-lungs, as also sundry other sea-animals that resemble them. For these are free and unattached, yet they have no feeling, and their life is simply that of a plant separated from the ground. For even among land-plants there are some that are independent of the soil . . . or even entirely free. Such, for example, is the plant which is found on Parnassus, and which some call the Epipetrum. This you may hang up on a peg and it will yet live for a considerable time. Sometimes it is a matter of doubt whether a given organism should be classed with plants or with animals. The Tethya, for instance, and the like so far resemble plants as they never live free and unattached, but, on the other hand, inasmuch as they have a certain flesh-like substance, they must be supposed to possess some degree of sensibility.[2]

[1] *Historia animalium*, viii. 1 ; $588^b$ 4.

[2] *De partibus animalium*, iv. 5 ; $681^a$ 15.

'The Acalephae or Sea-nettles, as they are variously called, . . . lie outside the recognized groups. Their constitution, like that of the Tethya, approximates them on the one side to plants, on the other side to animals. For seeing that some of them can detach themselves and can fasten on their food, and that they are sensible of objects which come in contact with them, they must be considered to have an animal nature. The like conclusion follows from their using the asperity of their bodies as a protection against their enemies. But, on the other hand, they are closely allied to plants, firstly by the imperfection of their structures, secondly by their being able to attach themselves to the rocks, which they do with great rapidity, and lastly by their having no visible residuum notwithstanding that they possess a mouth.' [1]

Thus 'Nature passes from lifeless objects to animals in such unbroken sequence, interposing between them beings which live and yet are not animals, that scarcely any difference seems to exist between two neighbouring groups owing to their close proximity.' [2]

'In regard to sensibility, some animals give no indication whatsoever of it, whilst others indicate it but indistinctly. Further, the substance of some of these intermediate creatures is flesh-like, as is the case with the so-called Tethya (ascidians) and the Acalephae (or sea anemones ?) ; but the sponge is in every respect like a vegetable. And so throughout the entire animal scale there is a graduated differentiation in amount of vitality and in capacity for motion. A similar statement holds good with regard to habits of life. Thus, of plants that spring from seed, the one function seems to be the reproduction of their own particular species, and the sphere of action with certain animals is similarly limited. The faculty of reproduction, then, is common to all alike. If sensibility be superadded, then their lives will differ from one another in respect to sexual intercourse and also in regard to modes of parturition and ways of rearing their young. Some animals, like plants, simply procreate their own species at definite seasons ; other animals busy themselves also in procuring food for their young, and after they are reared quit them and have no further dealings with them ; other animals are more intelligent and endowed with memory, and they live with their offspring for a longer period and on a more social footing.' [3]

### (c) *Ontogeny*

So much for Aristotle's treatment of the kinds of living things. Evolutionary doctrine is also foreshadowed by him in his theories of the development of the individual. This fact is obscured, however, by his peculiar view of the nature of procreation. On

[1] *De partibus animalium*, iv. 5 ; 681$^{a}$ 36.

[2] *De partibus animalium*, iv. 5 ; 681$^{a}$ 10.

[3] *Historia animalium*, viii. 1 ; 588$^{b}$ 16.

this topic his general conclusion is that the material substance of the embryo is contributed by the female, but that this is mere passive formable material, almost as though it were the soil in which the embryo grows. The male contributes the essential generative agency, but it is not theoretically necessary for anything material to pass from male to female. The material that does in fact pass with the seed of the male is an accident, not an essential, for his essential contribution is not matter but *form* and *principle*. Aristotle, it appears, was prepared to accept instances of fertilization without material contact.

'The female does not contribute semen to generation but does contribute something . . . for there must needs be that which generates and that from which it generates. . . . If, then, the male stands for the effective and active, and the female considered as female, for the passive, it follows that what the female would contribute to the semen of the male would not be semen but material for the semen to work upon. . . . Now how is it that the male contributes to generation, and how is it that the semen from the male is the cause of the offspring ? Does it exist in the body of the embryo as a part of it from the first, mingling with the material which comes from the female ? Or does the semen contribute nothing to the material body of the embryo but only to the power and movement in it ? For this power is that which acts and makes, while that which is made and receives the form is the residue of the secretion in the female. Now the latter alternative appears to be the right one both *a priori* and in view of the facts.' [1]

There was, however, another view of generation, perhaps of Epicurean origin, that was prevalent in antiquity. According to this theory the foetus was a joint product of male semen and of some analogous factor secreted by the female.[2] Among later writers, from Galen onward, the Aristotelian and Epicurean views were often blended and confused. After the thirteenth century, however, the Aristotelian doctrine was that mainly held and it lasted on until quite modern times. Thus it profoundly influenced William Harvey in the seventeenth century. With the discovery of the spermatozoa, however, by Leeuwenhoek and Hamm, in 1677, it became substantially untenable. The view of Aristotle fell altogether into discredit in the nineteenth century, during the

[1] *De generatione animalium*, i. 21 ; 729$^a$ 21.

[2] The theory is succinctly stated by Lucretius, iv, ll. 1229–31 :

> Semper enim partus duplici de semine constat,
> atque utri similest magis id quodcumque creatur,
> eius habet plus parte aequa.

long period of what may be called histological domination. We have now, however, entered on a new experimental period in biology, and recent work on mechanical stimulus of the ovum has demonstrated that it is indeed possible for development to proceed without passage of material from the male.

Aristotle's most important embryological researches were made upon the chick. He says that the first signs of development are noticeable after three days, the heart being visible as a palpitating blood spot whence, as it develops, two meandering blood-vessels extend to the surrounding tunics.

'Generation from the egg', he says, 'proceeds in an identical manner with all birds, but the full periods from conception to birth differ. With the common hen after three days and nights there is the first indication of the embryo. . . . The heart appears like a speck of blood, in the white of the egg. This point beats and moves as though endowed with life, and from it two vein-ducts with blood in them trend in a convoluted course . . . and a membrane carrying bloody fibres now envelops the yolk, leading off from the vein-ducts.' [1]

A little later he observed that the body had become distinguishable, and was at first very small and white.

'The head is clearly distinguished and in it the eyes, swollen out to a great extent. This condition of the eyes lasts on for a good while, as it is only by degrees that they diminish in size and collapse. At the outset the under portion of the body appears insignificant in comparison with the upper portion. Of the two ducts that lead from the heart, the one proceeds towards the circumjacent integument, and the other, like a navel-string, towards the yolk. . . .

'When an egg is ten days old the chick and all its parts are distinctly visible. The head is still larger than the rest of the body and the eyes larger than the head. At this time also the larger internal organs are visible, as also the stomach and the arrangement of the viscera; and the veins that seem to proceed from the heart are now close to the navel. From the navel there stretch a pair of veins, one towards the membrane that envelops the yolk and the other towards that membrane which envelops collectively the membrane wherein the chick lies, the membrane of the yolk, and the intervening liquid. . . . About the twentieth day, if you open the egg and touch the chick, it moves inside and chirps; and it is already coming to be covered with down, when, after the twentieth day is past, the chick begins to break the shell.' [2]

[1] *Historia animalium*, vi. 3; 561$^a$ 4. [2] *Historia animalium*, vi. 3; 561$^a$ 18.

To realize what must have been Aristotle's impressions on seeing the developing chick we should not only eliminate our own embryological prepossessions but should also divest ourselves of such modern conveniences of a laboratory as incubator, water-bath, and microscope, and even lens. For our purpose of comparison, better than the description of a modern text-book of embryology is the account of such a pioneer embryologist as Fabricius ab Aquapendente (1537–1619) whose work was done before the microscope had come into use (Fig. 5). Fabricius

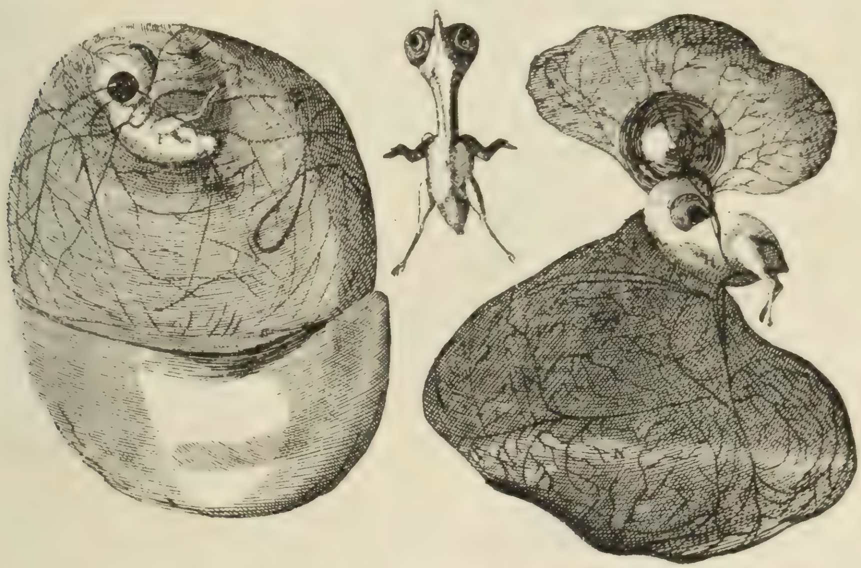


FIG. 5. THE YOUNG CHICK
From Fabricius ab Aquapendente's *De formatione ovi et pulli*, Padua, 1604.

was not, it is true, the first of modern writers to make embryological observations; for that position Leonardo da Vinci is perhaps the most likely candidate. Rieff (1500–58) also, in a work for midwives (1554), had written matter concerning the development of the human embryo that was not without point.[1] Moreover, Coiter (1534–1590) had already (1573) discussed the incubated fowl's egg[2] in a manner that displayed an understanding of the nature of the vitelline duct to which neither Fabricius nor even

[1] Jacob Rieff, *Trostbüchle*, Zurich, 1554. There is a Latin edition of this work (translated by Wolfgang Haller ?), *De conceptu et generatione hominis*, Zurich, 1554, and an excellent anonymous English translation, *The Expert Midwife*, London, 1637.

[2] Volcher Coiter, *Externarum et internarum principalium humani corporis partium tabulae, atque anatomicae exercitationes observationesque variae diversis ac artificiossissimis figuris illustratae*, Nuremberg, 1573.

Harvey altogether attained,[1] although it had been grasped by Aristotle.[2] But Fabricius was the first to imitate Aristotle in making extensive embryological observations under controlled conditions.[3]

Like Aristotle, Fabricius carried on his researches without means of magnification; like him he did his work almost without help from previous observers; following Aristotle, he held that there was a fundamental distinction between the male and female contribution to the formation of the embryo in the case of the viviparous placental animals, and as with Aristotle, the ovum of mammals was unknown to him, though its existence was suspected by his pupil Harvey. For these pioneers of embryology the *semen* or *sperm* was indeed literally the *seed*, the fertilizing principle—the word used for the seed of animals was the same as that used for plants both in Latin and in Greek—a seed which was sown in the soil of the mother's womb where it was nourished and where it grew. Harvey in his work on Generation suggested, without full evidence, that 'almost all animals, even those which bring forth their young alive and man himself are produced from eggs',[4] but it was not until the last quarter of the seventeenth century and the appearance of the work of de Graaf[5] (Figs. 6 and 7), Swammerdam,[6] and Stensen[7] that the notion came clearly into view that the so-

[1] In modern times the nature of the vitelline duct was first described by Coiter, but his work was overlooked until his observations were repeated by Stensen in his *De musculis et glandulis observationum specimen*, Copenhagen, 1664.

[2] Aristotle, *Historia animalium*, vi. 3.

[3] Hieronymus Fabricius ab Aquapendente, *De formatione ovi et pulli*, Padua, 1604.

[4] The phrase *omne vivum ex ovo*, sometimes attributed to Harvey, cannot, however, actually be found in his writings.

[5] Regnier de Graaf, *De mulierum organis generationi inservientibus tractatus novus, demonstrans tam homines et animalia, caetera omnia, quae vivipara dicuntur, haud minus quam ovipara, ab ovo originem ducere*, Leyden, 1672.

[6] Jan Swammerdam, *Miraculum naturae sive uter muliebris fabrica*, Leyden, 1672.

[7] In 1667 Stensen published his *Elementorum myologiae specimen . . . cui accedunt canis Carchariae dissectum caput, et dissectus piscis ex canum genere* at Florence, reprinted Amsterdam, 1669. In the third treatise of this work he maintained that the *testes* of women were analogous to the *ovaria* of oviparous animals and ought to be called by that name. In 1675 he published at Copenhagen in the *Acta Hafniensia* his *Observationes anatomicae spectantes ova viviparorum* in which he briefly described ova in a variety of viviparous animals. In an adjacent publication of the same year of the *Acta Hafniensia* entitled *Ova viviparorum spectantes observationes* he gives a diagram illustrating the analogy

called 'testes' of the female really contained eggs or ova comparable to the ova of oviparous creatures. In the absence of this knowledge Aristotle, like Fabricius, was unable to set forth an

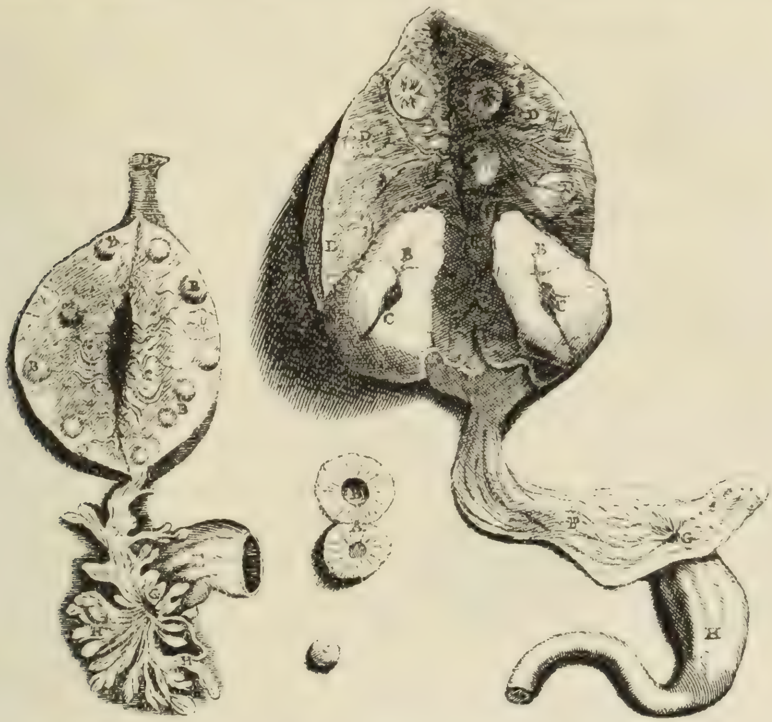


FIG. 6. From Regnier de Graaf's *De mulierum organis generationi inservientibus*, Leyden, 1672. To the left is the 'testicle or ovary', as he calls it, of a woman. It is cut open along the line A; BB are 'ova' of various sizes contained in the substance of the 'testis'; CC are blood vessels; D is the ligament of the ovary; E a part of the Fallopian tube and G its opening; H and I are the *ornamenta foliacea tubarum.* To the right is the ovary of a cow similarly cut open along the line AA; BB is the *Glandulosa substantia, quae post ovi expulsionem in testibus reperitur*, or as we call it the *corpus luteum*, CC being the 'almost obliterated cavity' in which the ovum was once contained; DD are ova; EE blood vessels; F, G, H the Fallopian tube. Between the two larger figures is the Graafian follicle AB of a sheep, C being the ovum removed from it.

explanation of the mechanism of generation that adequately covered the phenomena.

of the uteri of *vipara* and *ovipara*. Thus Stensen has the priority in the *suggestion* that the testes of the female mammal produce ova but de Graaf has the priority of *demonstration*.

The claim disseminated by A. Portal in his *Histoire de l'Anatomie et de la Chirurgie*, 6 vols., Paris, 1770–1773, that Jean Mathieu Ferrari da Grado (fl. 1450) was the discoverer of the ovarian nature of the female *testes*, is effectually disposed of by da Grado's collateral descendant H. M. Ferrari, in his *Une chaire de Médecine au XVᵉ siècle*, Paris, 1899, p. 115 ff.

Aristotle, true to the general gradational view that he had formed of Nature, held that the most primitive and fundamentally important organs make their appearance before the others. Among the organs all give place to the heart, which he considered 'the first to live and the last to die'.[1] Here again he was followed by the great investigators of the newly revived experimental method. Harvey has enshrined this idea in his work on the circulation. Indeed the conception of a hierarchy of the organs hardly departed from Biology until the observations of Caspar Friedrich Wolff (1733–94) and Karl Ernst von Baer (1792–1876) had been rationalized by Darwin, so that physiologists could at last turn from the consideration of the organ to a contemplation of the organism, and naturalists became enabled to think of the individual not as what it is but as what it has been and what it is becoming. Aristotle's kinetic view was at length vindicated.[2]

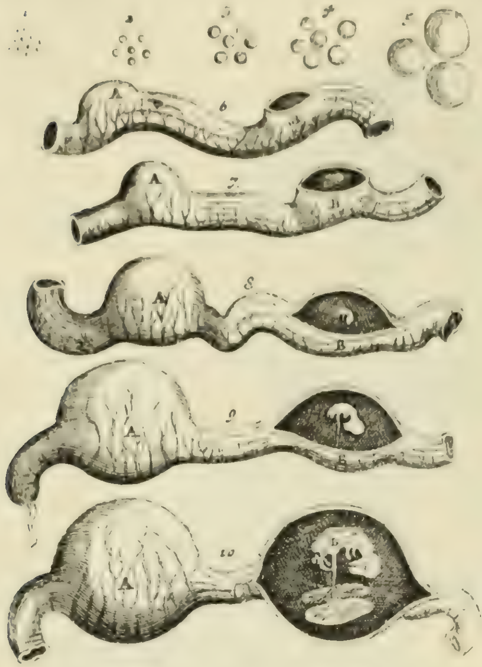


FIG. 7. From Regnier de Graaf's *De Mulierum organis generationi inservientibus*, Leyden, 1672. Illustrating the development of the rabbit's ovum: 1, ova on the third day after conception, 2 on the fourth, 3 on the fifth, 4 on the sixth, and 5 on the seventh day. The remaining figures show a section of the tube containing two embryos, 6 being on the eighth and 10 on the fourteenth day. The last figure shows the placenta.

[1] This is the sense of Aristotle, e.g. *De generatione animalium*, ii, 1 and 4; 735ᵃ 25 and 738ᵇ 16. The phrase, however, *primum vivens ultimum moriens* is, I think, first used in Latin translations of Averroes (1126–98), the commentator on Aristotle. There is a discussion of the origin of the phrase in the *Mitteilungen z. Gesch. der Med. und Naturwissenschaften*, xix, pp. 102, 219, and 305, Leipzig, 1920.

[2] There is a discussion of ancient embryological literature by Bruno Bloch, 'Die geschichtlichen Grundlagen der Embryologie bis auf Harvey', in the *Abhandlungen der kais. Leopold.-Carol. Akad.*, lxxxii, pp. 213–334, Halle, 1904. There is a shorter version of this same article in the *Zoologische Annalen*, i, p. 51, Würzburg, 1905.

## IV. Some Aristotelian Zoological Observations and their Modern Counterparts

### (a) *The Placental Shark*

We may now turn to observations in the Aristotelian writings on the habits of animals and on comparative anatomy. These are far too numerous for extended consideration here, and we therefore select a few that are of special historical interest for comparison with their modern counterparts.

Aristotle recognized a distinction in the mode of development of mammals from that of other viviparous creatures. Having distinguished the apparently viviparous animals as either truly and internally or merely externally viviparous, he pointed out that in the mammalia, a group regarded by him as internally viviparous, the foetus is connected until birth with the wall of the mother's womb by the navel string. These animals, in his view, produced their young without the intervention of an ovum. Such non-mammals, on the other hand, as are viviparous are so in the external sense only, that is, the young which arise from ova may indeed develop within the mother's body, but they do so out of organic connexion with her, so that her womb acts, as it were, but as a nursery or incubator for her eggs. It was thus a sort of accident whether in a particular species the ova went through their development inside or outside the mother's body. 'Some of the ovipara', he says, 'produce the egg in a perfect, others in an imperfect state, but it is perfected outside the body as has been stated of fish.' [1]

But it is exceedingly interesting to observe that although Aristotle regarded fishes as a whole as oviparous, he knew also of kinds that were externally viviparous and he knew, further, of one instance in which the manner of development bore an analogy to that of his true internal vivipara. 'Some animals', he says, 'are viviparous, others oviparous, others vermiparous. Some are viviparous, such as man, the horse, the seal and all other animals that are hair-coated, and, of marine animals, the cetaceans, as the dolphin, *and the so-called Selachia.* Of these animals, some have a tubular air-passage and no gills as the dolphin and the whale, others have uncovered gills, as the Selachia, the sharks and rays. . . . Of viviparous animals some hatch eggs in their own interior as creatures of the shark kind ; others engender in their interior a live foetus, as man and the horse.' [2]

[1] *De Generatione animalium*, iii. 9 ; 758ᵃ 37. [2] *Historia animalium*, i. 5 ; 489ᵇ 35.

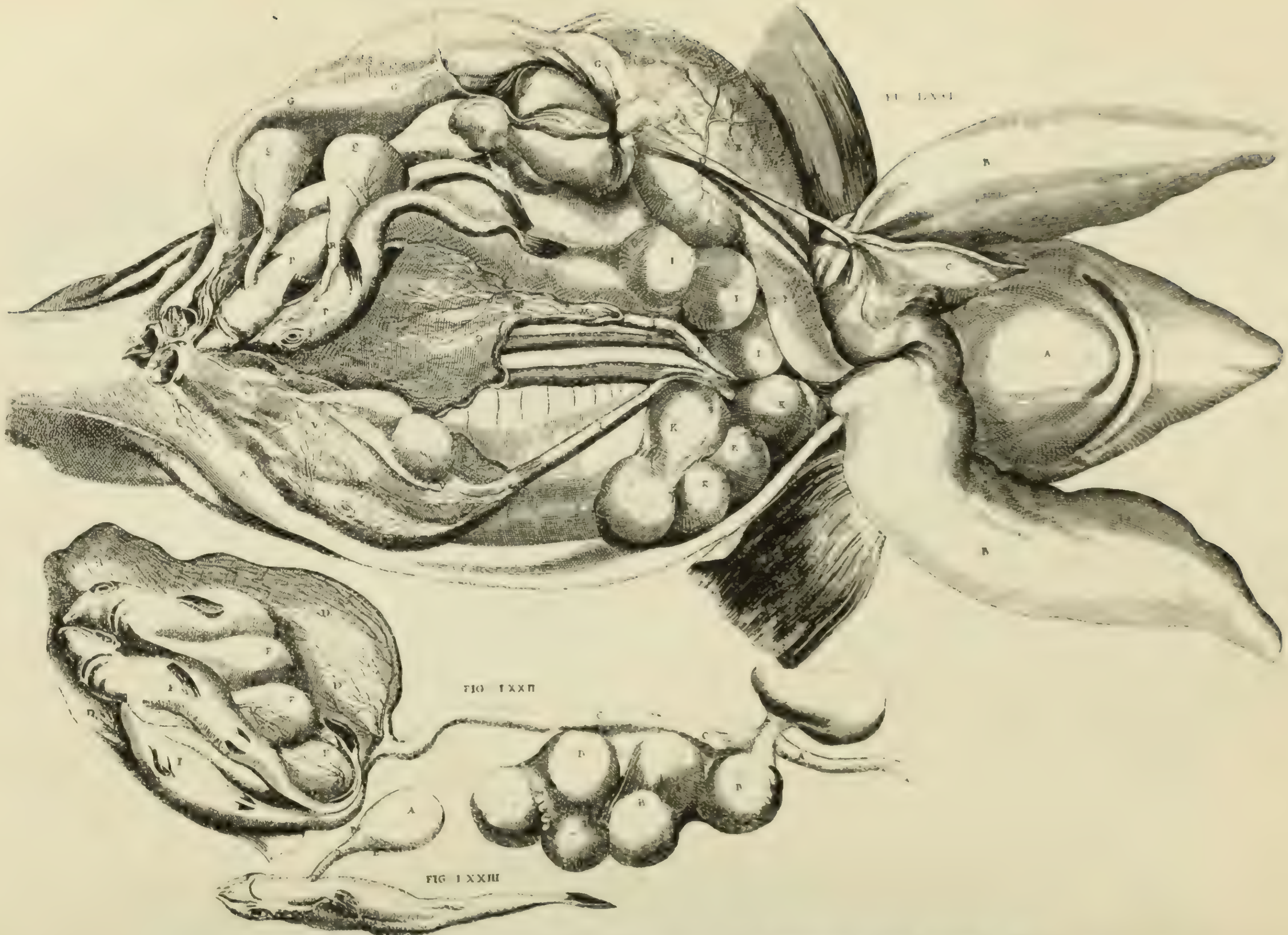



Fig. 8. From Fabricius ab Aquapendente, *De Formato Foetu*, Padua, 1604.

LXXI. Female *Galeus laevis* opened to show the gravid uterus. A, head of fish; BB, lobes of liver; C, gall bladder; D, bile duct; E, stomach; GG, intestine; II and KK, ova; LMNO, Fallopian tube; PP, foetuses in the uterus with yolk sacs, QQ, attached by the umbilical cords RRSS; X and Y are the uterine orifices, and Z the rectal opening.

LXXII. A uterus opened exhibiting three foetuses. BB, ova; CC, tube; DD, uterus; EEE, the foetuses, and FFF, their yolk sacs.

He even attempts to give an explanation of this peculiarity of the Selachians. His explanation may seem to modern ears to have little meaning, just as many of our scientific explanations will seem meaningless to our successors in a generation or two. But such explanations are worth recording not only as a stage in the historical development of biological theory, but also as illustrating the fact that, in those days as in these, while the function of science is the *description* of nature its motive is almost always the *explanation* of nature. Yet it is usually the descriptive, not the explanatory element that bears the test of time. 'Birds and scaly reptiles', says Aristotle, 'because of their heat produce a perfect egg, but because of their dryness, it is *only* an egg, the cartilaginous fishes have less heat than these but more moisture, so that they are intermediate, for they are both oviparous and viviparous within themselves, the former because they are cold, the latter because of their moisture; for moisture is vivifying, whereas dryness is furthest removed from what has life. Since they have neither feathers nor scales such as either reptiles or other fishes have, all which are signs rather of a dry and earthy nature, the egg they produce is soft; for the earthy matter does not come to the surface in their eggs any more than in themselves. That is why they lay eggs in themselves, for if the egg were laid externally it would be destroyed, having no protection.' [1]

This explanation is, of course, based on his fundamental doctrine of the opposite *qualities*, heat, cold, moistness and dryness that are found combined in pairs in the four *elements*, earth, air, fire, and water.

The intermediate character of the Selachians between the viviparous and the oviparous, as set forth by Aristotle, was well brought out by Fabricius ab Aquapendente [2] who described and figured young dogfish attached each to its own yolk sac and developing within the uterus of the mother (Fig. 8). But Aristotle had carried his investigation farther than Fabricius, for he knew of that small group of Selachian species in which the method of nourishment of the young presents remarkable analogies to that of the placental mammals. In this group of fishes the wall of the yolk sac becomes thickened at one point and attached to a corresponding thickening in the wall of the uterus. In this way a 'placenta' is formed very similar to, though not homologous with, the mammalian placenta, and the little developing fish derives nutriment from the mother's body through the placenta and navel-string much as in a mammal.

[1] *De generatione animalium*, ii. 1; 733$^a$ 6.

[2] Hieronymus Fabricius ab Aquapendente, *De formato foetu*, Padua, 1604.

'The so-called smooth shark', says Aristotle—*Galeos* he calls it, and the name is still used by Greek fishermen—'has its eggs in betwixt the wombs like the dog-fish; these eggs shift into each of the two horns of the womb and descend, and the young develop with the navel-string attached to the womb, so that as the egg-substance gets used up, the embryo is sustained to all appearance just as in quadrupeds. The navel-string is long and adheres to the under part of the womb—each navel-string being attached as it were by a sucker, and also to the centre of the embryo in the place where the liver is situated. If the embryo is cut open, even though it has the egg-substance no longer, the food inside is egg-like in appearance. Each embryo, as in the case of quadrupeds, is provided with a chorion and separate membranes.'[1]

The attachment of the young Selachian to the womb of its mother was first observed in modern times by Pierre Belon

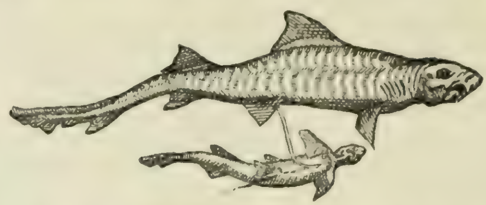


FIG. 9. *Galeus laevis*, from Rondelet's *De piscibus marinis*, Lyons, 1554.

'We have had an illustration made', says Rondelet, 'of the young attached by the navel cord to the mother so that it may be distinguished from sea-dogs, sea-wolves and the other sharks, as there is no other shark whose young is covered with secundines and membranes and attached to the mother by a navel string. I am aware', he continues, 'that there is another shark with a smoother skin than this: yet, as its manner of reproduction differs from that just described, I assert that it is not the *Galeus laevis* of the ancients but rather the *Galeus glaucus* of Aelian. [*De nat. animal.* i. 16.]'

(1517–1654) in 1553[2] and roughly figured by Guillaume Rondelet (1507–66) in 1554[3] (Fig. 9). A somewhat similar account was given by the missionary du Terte, in 1667.[4] The description of Belon was copied by Aldrovando (1522–1607) in a work published in 1613[5] and this came to the knowledge of Stensen, who definitely determined the relationship of the Selachian embryo to the wall of the maternal womb in 1675.

'In the *Galeus laevis*', says Stensen, 'each foetus has its own membrane which may be regarded as the amnion, since like the

[1] *Historia animalium*, vi. 10; $566^b$ 2.

[2] Pierre Belon, *De aquatilibus cum iconibus ad vivam ipsorum effigiem quod fieri potuit*, Paris, 1553, p. 69.

[3] Guillaume Rondelet, *De piscibus marinis*, Lyons, 1554.

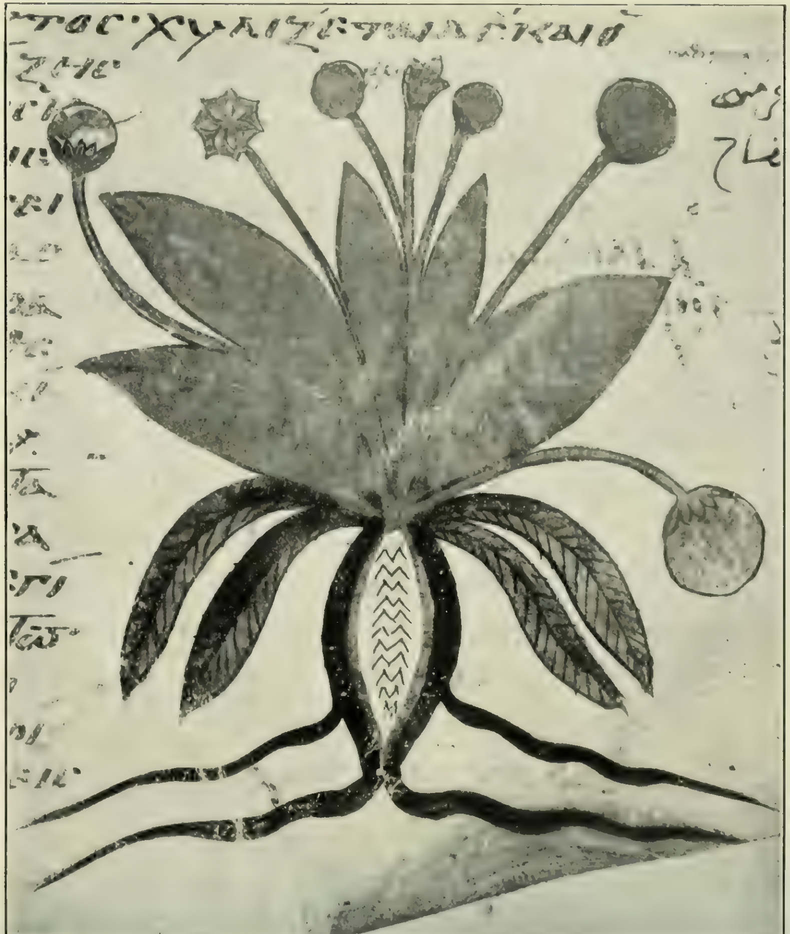
[4] Jacques du Terte, *Histoire générale des Antilles habitées par les Français*, 4 vols., Paris, 1667.

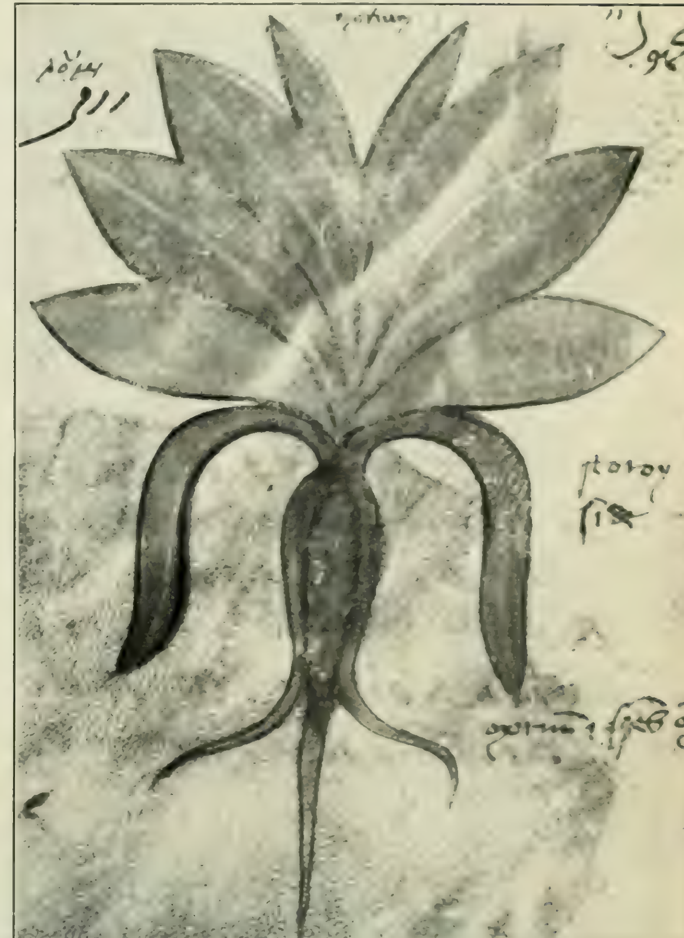
[5] Ulisse Aldrovando, *De piscibus*, Bologna, 1613, p. 375.


fo. 20r χαμηλή = Bugle? ἄκανθος = Acanthus


fo. 16v ἀλκίβιον = Anchusa officinalis?

Plate X. Paris Bib. nat. MS. grec 2179 Dioscorides IXth cent.





fo. 104 r 'MALE' AND 'FEMALE' MANDRAKES fo. 105 r

amnion it is the covering of the foetus and floats in the clear liquid. Yet it differs from the amnion in that it is united to the placenta in the way characteristic of the chorion (in mammals). . . . There is only a single very small placenta to each foetus, red in colour and situated close to the lower orifice of the yolk and a membrane drawn over forms a cavity.

'The umbilical vessels pass into the abdomen of the foetus by a channel beneath the diaphragm between the two anterior lobes. By following this duct I observed air bubbles floating in the intermediate liquid. On being propelled these disappeared into the intestine beyond. Next the intestine of a second foetus was inflated, and while I was moving it in different directions I opened a way for the air towards the placenta. Thus it was evident that a non-vascular tube was included among the *vasa umbilicalia*; of this vessel one extremity was joined to the spiral intestine within the abdomen, the other to the placenta where its upper surface forms a cavity with a thin membrane covering it. From the structure of the tube it is evident that nourishment is brought to the intestine from the cavity of the placenta in this fish as in birds from the yolk, as long as food is supplied to the foetus by the humours of the mother.'[1] (Fig. 10.)

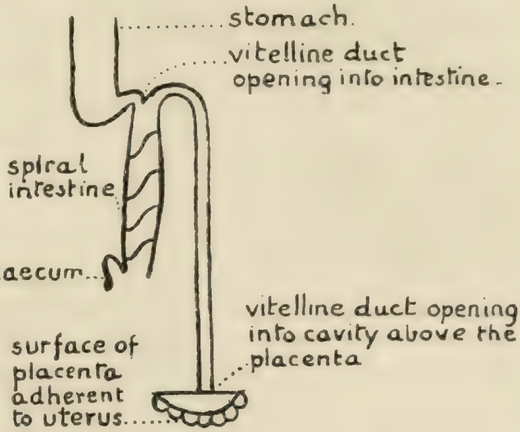



FIG. 10. After Stensen's diagram in *Ova viviparorum spectantes observationes*, Copenhagen, 1675, showing relation of yolk sac to umbilical cord and intestine.

The observations of Stensen were long disregarded. In 1828, Cuvier in his great work on fishes[2] remarked briefly that in Carcharias the yolk sac is attached to the uterus as firmly as a placenta. Neither Stensen nor Cuvier referred to Aristotle on this subject, and the importance of the ancient observations was unappreciated until the greatest of modern morphologists, Johannes Müller, took up the subject in 1839. Müller made it clear that there are at least two genera of Selachians in which this peculiar placental development takes place, namely *Carcharias* and *Mustelus* (Figs. 11–

[1] Nicolaus Stensen, *Ova viviparorum spectantes observationes*, in T. Bartholin's *Acta Hafniensia*, 1675. The works of Stensen have been made accessible by Vilhelm Maar in his *Nicolai Stenonis Opera philosophica*, 2 vols., Copenhagen, 1910. Cf. ii, p. 169.

[2] Georges Cuvier, *Histoire naturelle des Poissons*, Paris, 1828, vol. i, p. 341.

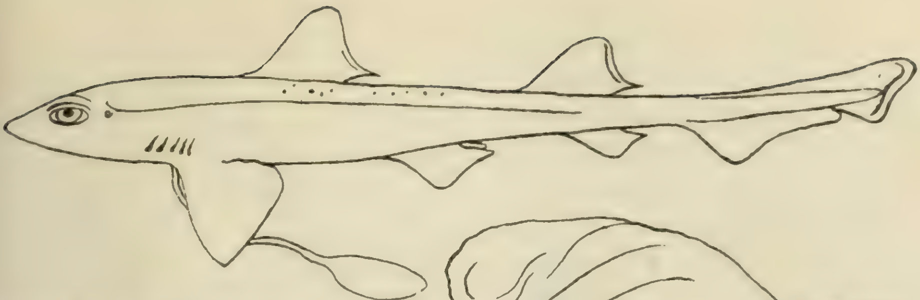
15). There can be little doubt from Aristotle's descriptions that his *Galeos leios* was not a large shark like *Carcharias* but a smaller dogfish answering to Müller's *Mustelus*. Müller further demonstrated the very peculiar fact that within the genus *Mustelus* one species (*M. laevis*) has the foetus firmly united to the uterus by means of a placenta, while in another closely allied species (*M. vulgaris*) the yolk sac is quite free and unattached. It is interesting to observe that the distinction between the two allied species was quite accurately made in 1554 by Rondelet (cf. legend to Fig. 9).

Johannes Müller describes the placenta of *Carcharias* (Figs. 12 and 13) in greater detail than that of *Mustelus* (Figs. 14 and 15).

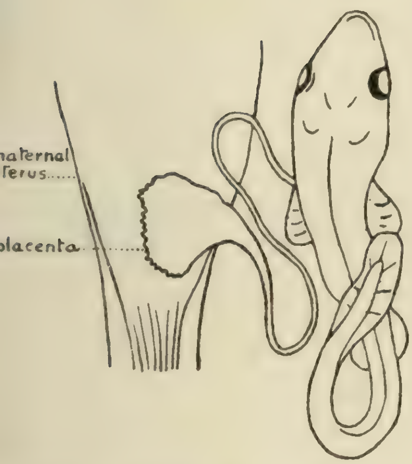
'The *placenta foetalis* of these fish', he says, 'is formed by the folded yolk sac. The folds are much more complex in *Carcharias* than in *Mustelus laevis*. . . . In *Carcharias* the yolk sac, as usual, possesses two coats, an internal vascular coat, continuous with the intestine through the yolk duct, and an external non-vascular coat extending as a sheath over the yolk duct and the omphalo-meseraic vessels, and continuous with the skin of the foetus. . . . In the formation of the placenta both membranes are thrown into a mass of folds and the yolk sac thus converted into an irregular cavity. These folds on the side approximated to the uterus become closely involved with the wall of that organ and cannot be separated except by some force. On the other hand the part of the yolk sac on the side away from the uterus presents mere floating diverticula. Over the placental area the two walls of the yolk sac are in closest contact with each other, but elsewhere the membranes are separated from each other by a distinct interval. . . .

'The *placenta uterina* is formed by very prominent wrinkled folds of the inner membrane of the uterus which accurately correspond to those of the *placenta foetalis*. The folds of both are interposed between each other, and are as closely and firmly attached as the placenta uterina and placenta foetalis of any mammiferous animal. . . . The placenta uterina receives its blood-vessels from the uterine vessels, which are of large size, and run to the seat of the placenta at the lower part of the organ. The vessels of the placenta foetalis are the extraordinarily large *vasa omphalo-mesaraica* which are proportionally of as great size as the *vasa umbilicalia* of mammals. The organic relation of placenta foetalis and placenta uterina to one another is the same as in mammals.' [1]

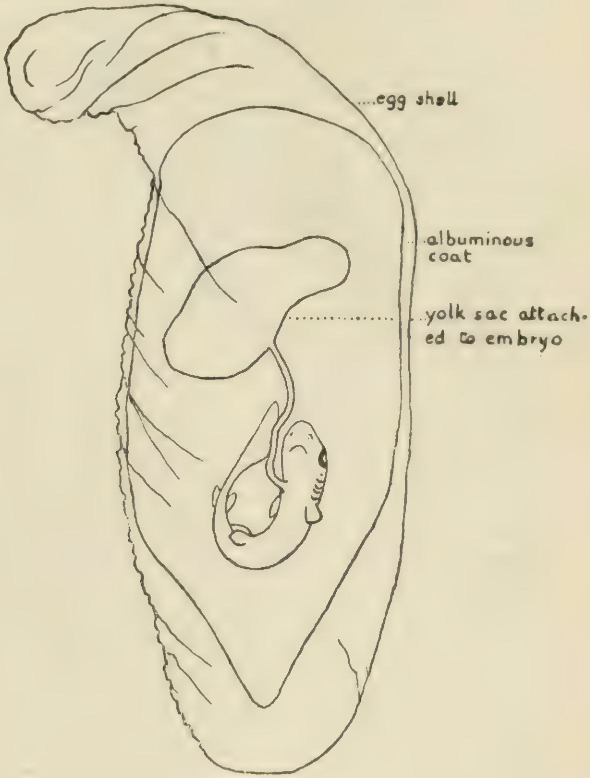
[1] Johannes Müller, *Handbuch der Physiologie des Menschen*, 2 vols., Coblenz, 1840, vol. ii, p. 722. See also translation by William Baly, *Embryology, with the Physiology of Generation*, London, 1848, p. 1597, and Johannes Müller in *Monatsbericht der Akad. der Wissenschaften zu Berlin*, August 6, 1840, *Ueber den glatten Hai des Aristoteles*, Berlin, 1842, and in *Monatsbericht d. Berlin. Akad.*, 11th April, 1839.



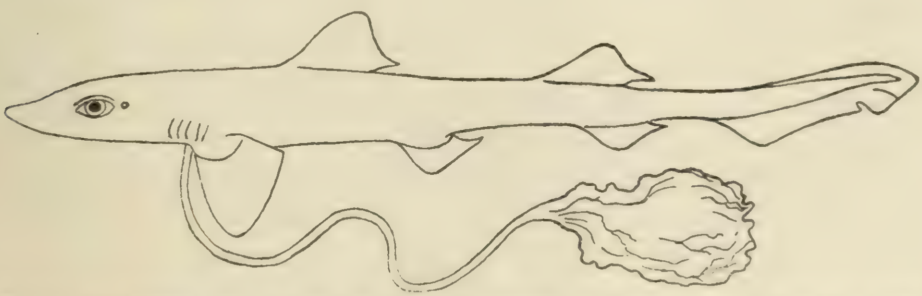
Embryo of *Mustelus vulgaris*, 7 lines long, with yolk sac.




Embryo of *Mustelus laevis* in connexion with the uterus.




Egg of *Mustelus vulgaris.*



Embryo of *Mustelus laevis*, 7 lines long, with the placental yolk sac separated from the uterus.

FIG. 11. Embryos of two species of *Mustelus*. From Johannes Müller, *Ueber den glatten Hai des Aristoteles*, Berlin, 1842.

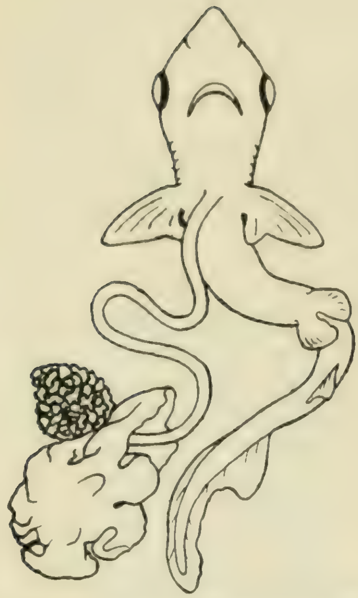


FIG. 12. Embryo of *Carcharias* with umbilical cord and placenta. From Johannes Müller.

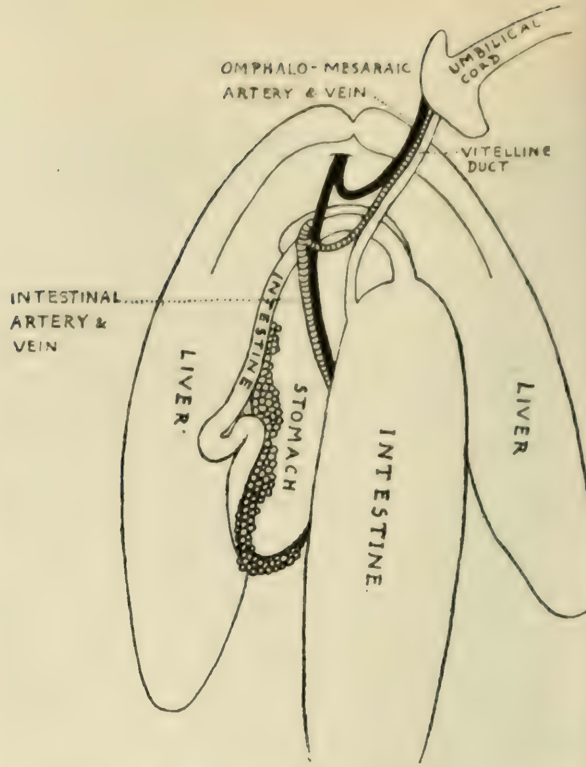



FIG. 13. Dissection of umbilical structures of a foetal *Carcharias*, schematically represented. Modified from Johannes Müller.

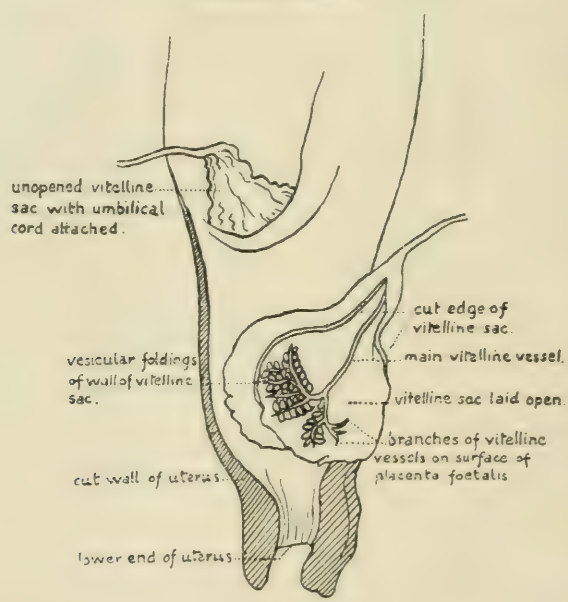



FIG. 14. A part of the uterus of *Mustelus laevis*, showing two placental attachments. From Johannes Müller.

Since Müller wrote, other observers have brought the phenomenon of the placenta of *Mustelus* and *Carcharias* more into line with what we know of allied viviparous forms. The embryos of many of these, when the yolk is nearly consumed, are nourished from other sources. In some the nutritive material is secreted

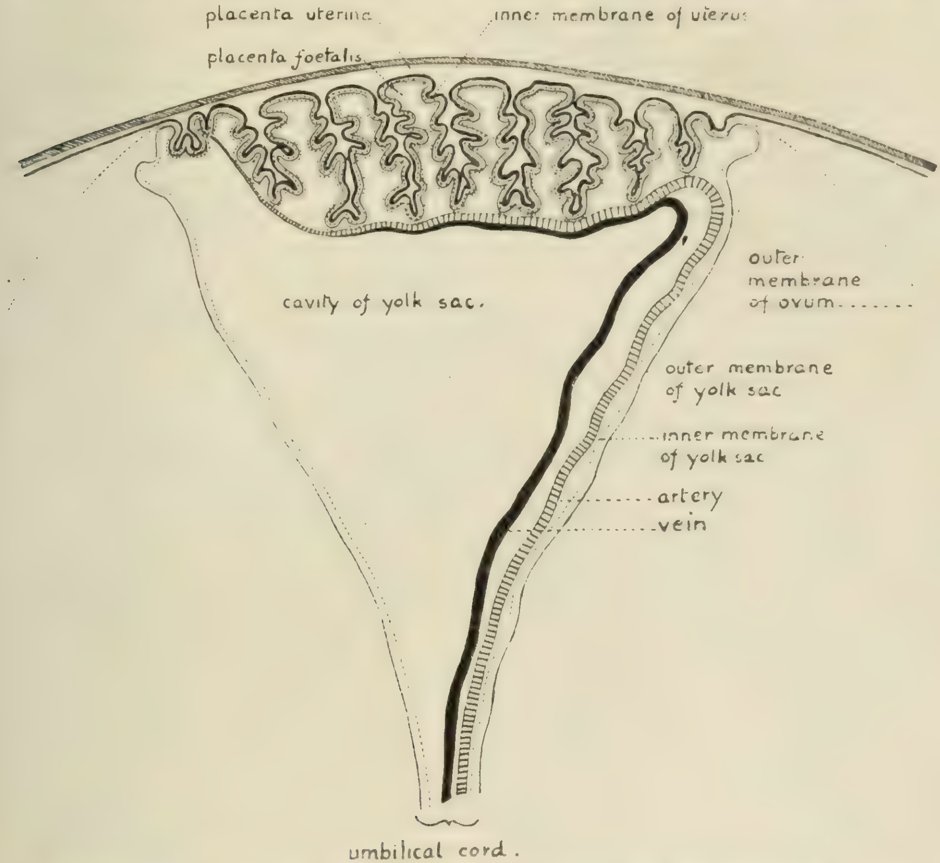



Fig. 15. DIAGRAMMATIC SECTION OF PLACENTA OF *MUSTELUS LAEVIS*. Modified from Johannes Müller.

into the cavity of the uterus and taken into the mouth of the embryo or absorbed by the blood-vessels of the yolk sac or of the gill slits. In others the wall of the uterus develops secreting villi which pass through the spiracles of the embryo into its pharynx. In yet others in which a placenta is actually developed the process of absorption is aided by villi which stud the umbilical cord throughout its course.[1]

[1] These accessory methods are described in the following papers which contain a bibliography of the subject: Franz Leydig, *Beiträge zur mikroskopischen Anatomie und Entwickelungsgeschichte der Rochen und Haie*, Leipzig, 1852;

### (b) *The Ruminant Stomach*

Among the more remarkable of Aristotle's descriptions in the realm of comparative anatomy proper is that of the stomach of ruminants. He must have dissected these animals, for he gives a clear and correct account of the four chambers.

'Animals', he says, 'present diversities in the structure of their stomachs. Of the viviparous quadrupeds, such of the horned animals as are not equally furnished with teeth in both jaws are furnished with four such chambers. These animals are those that are said to chew the cud. In these animals the oesophagus extends from the mouth downwards along the lung, from the midriff to the *megale koilia* [*rumen*, or paunch], and this stomach is rough inside and semi-partitioned. And connected with it near to the entry of the oesophagus is what is called the *kekryphalos* [*reticulum*, or honeycomb bag]; for outside it is like the stomach, but inside it resembles a netted cap; and the kekryphalos is a good deal smaller than the megale koilia.'

The term *kekryphalos*, the reader may be reminded, was applied to the net that women wore over their hair to keep it in order.

'Connected with this kekryphalos', he continues, 'is the *echinos* [*psalterium*, or *manyplies*], rough inside and laminated, and of about the same size as the kekryphalos. Next after this comes what is called the *enystron* [*abomasum*], larger and longer than the echinos, furnished inside with numerous folds or ridges, large and smooth. After all this comes the gut.'[1] . . . 'All animals that have horns, the sheep for instance, the ox, the goat, the deer and the like have these several stomachs. . . . The several cavities receive the food one from the other in succession; the first taking the unreduced substances, the second the same when somewhat reduced, the third when reduction is complete, and the fourth when the whole has become a smooth pulp.'[2] . . . Such is the stomach of those quadrupeds that are horned and have an unsymmetrical dentition; and these animals differ one from

T. J. Parker and A. Liversidge, 'Note on the foetal membranes of *Mustelus antarcticus*', *Transactions of the New Zealand Institute*, xxii, p. 331, Wellington, 1890; J. Wood-Mason and A. Alcock, 'On the uterine villiform papillae of *Pteroplataea micrura*', *Proc. Roy. Soc.*, xlix, p. 359, London, 1891; J. Wood-Mason and A. Alcock, 'Further observations on the gestation of Indian Rays', *Proc. Roy. Soc.*, l, p. 202, London, 1892; A. Alcock, 'Some observations on the embryonic history of *Pteroplataea micrura*,' *Annals and Magazine of Natural History*, sixth series, x, p. 1, London, 1892; T. Southwell and B. Prashad, 'Embryological and Developmental Studies of Indian Fishes', *Records of the Indian Museum*, xvi, p. 215, Calcutta, 1919.

[1] *Historia animalium*, ii. 17; 507$^a$ 33.

[2] *De partibus animalium*, iii. 14; 674$^b$ 6.

PLATE XI. From Brit. Mus. MS. Reg. 15 E III fo. 11 r


Cy commence le douzieme
livre des proprietes des choses
ou il traitte premierement
des oyseaulx en general et
apres en particulier.

Duis que nous a
uons ou premier
volume determi
ne de lair et de
impressions qui y sont en
gendrees. maintenant ap
partient et nous reste de
dire aucune chose de ce qui
affiert a son mouvement.
A celle fin que la magnificen
ce et puissance du createur
soit en eulx loee et magni
fiee comme es autres crea
tures. ¶ A laornement
de lair donques appartient
les oyseaulx et toutes choses
qui volent. si comme dist
Bede le venerable. Et pour
ce a laide de dieu nous en
dirons vng pou de chose

*Le Livre des Propriétéz de Choses* translated by Jehan Corbechon from Latin of Bartholomew de Glanvil, written at Bruges by Jehan du Ries in 1482. Frontispiece of Book XII 'On Birds'.

Plate XII. PAINTINGS BY EDWARD TYSON (b. 1650, d. 1708) made in 1687

P. 41. Dissection of Lophius

P. 92. Stomach of Gazelle

From a MS. at the Royal College of Physicians, London

another in the shape and size of the parts, and in the fact of the oesophagus reaching the stomach centralwise in some cases and sideways in others. Animals that are furnished equally with teeth in both jaws have one stomach ; as man, the pig, the dog, the bear, the lion, the wolf.' [1]

The general appearance of the stomach of ruminants must always have been roughly known to butchers and its rediscovery cannot therefore be dated as can many of the biological observations of Aristotle that we have to recount. A fair scientific description of the organ was made by Aldrovando in 1613 [2] and by Fabricius in 1618.[3] The ruminant stomach was figured imperfectly by Severino in 1645 [4] (Fig. 16), and by Blasius in 1667.[5] There is a better figure by Grew of 1681 [6] (Fig. 17), and an excellent painting was prepared by Tyson, the earliest English comparative anatomist, in 1687 (Plate XII).

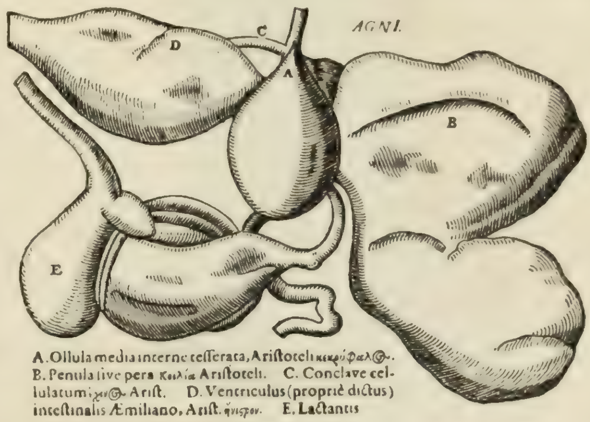



FIG. 16. THE FOUR CHAMBERS OF THE STOMACH OF A LAMB

From Marco Aurelio Severino, *Zootomia Democritea*, Nuremberg, 1645.

### (*c*) *The Generative processes of Cephalopods*

Nowhere is the contrast between the ancient and modern method of setting out biological conclusions better brought out than in the investigation of the extraordinarily interesting generative processes of the Cephalopods. An examination of the modern accounts of the subject enables us to observe the slow emergence of a true conception of the actual nature of the phenomena. This

[1] *Historia animalium*, ii. 17 ; $507^{b}$ 12.

[2] Ulisse Aldrovando, *Quadrupedium omnium bisulcorum historia*, Bologna, 1613.

[3] Hieronymo Fabricius, *De gula*, Padua, 1618.

[4] Marco Aurelio Severino, *Zootomia Democritea id est Anatome generalis totius animantium opificii, libris quinque distincta*, Nuremberg, 1645.

[5] Gerhard Blaes, *Observata anatomia*, Amsterdam, 1676, p. 49.

[6] Nehemiah Grew, *Catalogue of the rarities belonging to the Royal Society*, London, 1681.

process continues, in spite of many errors, because each observer records his actual observations and places them in such a form that the place, time and means of observation can be referred to at need, and the reader can himself separate what is observed from what is inferred and can grasp not only the nature of the observation itself but the means by which it was made. Thus the work of each writer can be criticized or modified by the next. In doing this both writer and reader are immeasurably aided by the use of figures. It is true that diagrams were used also by Aristotle,[1] but these appear to have been merely occasional devices rather than an intrinsic part of his method. Drawings or diagrams as routine aids to biological descriptions were probably uncommon until the first century B.C. In his account of the generative processes of the Cephalopods the ancient naturalist records only the final conclusions, and we hardly know which of the observations are his own and which are taken from

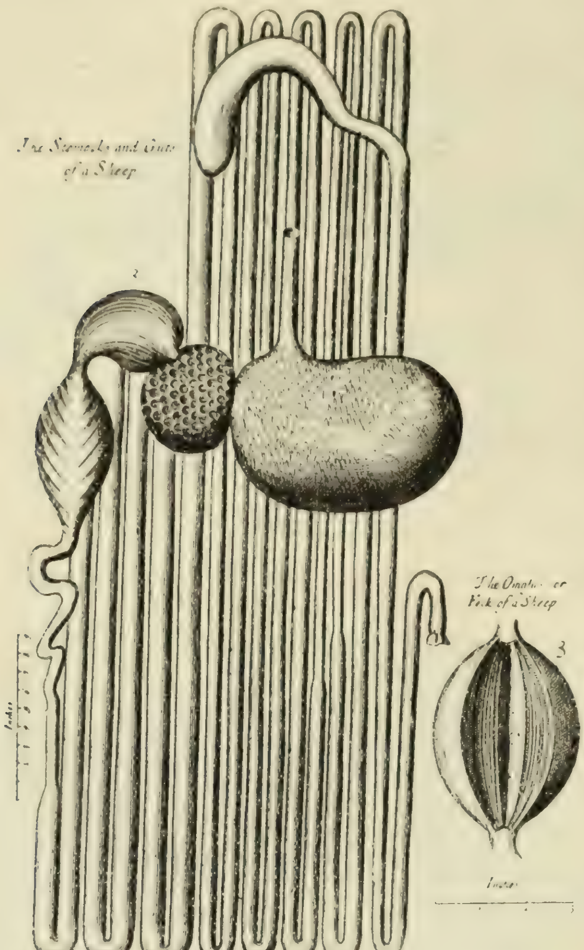


FIG. 17. THE FOUR-CHAMBERED STOMACH OF A SHEEP
After Nehemiah Grew, *The Comparative Anatomy of the Stomach and Guts Begun*, London, 1681.

[1] An interesting reference to the diagrams in Aristotle's lost work on Anatomy will be found in the *Historia animalium*, i. 17 ; 497ᵃ 33. Other references to anatomical diagrams are in the *De generatione animalium*, ii. 7 ; 746ᵃ 14, and the *Historia animalium*, iii. 1 ; 510ᵃ 29. The words used are σχήματα, διαγραφή, and παραδείγματα.

others, while he tells us nothing whatever of the conditions under which they were made. In spite of these faults in the record, his descriptions impel conviction that they are those of an acute and accurate observer, and that his work does not suffer from any lack in his powers as a naturalist. The main references to the reproduction of Cephalopods occur however in the ninth book of the *Historia animalium*, which is of more doubtful authenticity than the earlier parts.

With regard to the Cephalopod *Argonauta* Aristotle says that it is an

'octopus, but one peculiar both in its nature and habits.[1] . . . This polypus lives very often near to the shore, and is apt to be thrown up high and dry on the beach; under these circumstances it is found with its shell detached, and dies by and by on dry land.[2] . . . It rises up from deep water and swims on the surface. In between its feelers it has a certain amount of web-growth resembling the substance between the toes of web-footed birds; only that with these latter the substance is thick, while with the nautilus it is thin like a spider's web. (Cf. Plate XIII.) It uses this structure, when a breeze is blowing, for a sail, and lets down some of its feelers alongside as rudder-oars. (Cf. Fig. 18, p. 43.) If it be frightened it fills its shell with water and sinks. With regard to the mode of generation and the growth of the shell, knowledge is not yet satisfactory; the shell, however, does not appear to be there from the beginning, but to grow in their case as in that of the other shell-fish; neither is it ascertained for certain whether the animal can live when stripped of the shell.'[3]

The use of the membranes of the Argonaut as a sail and the arms as oars (Fig. 18) is now known to be pure myth, though many excellent naturalists, Vérany among them, have given colour to one or other of these ideas. It is but right to emphasize again that the ninth book of the *Historia animalium*, in which the statement occurs, is probably not the work of Aristotle himself.[4]

The questions that the Aristotelian treatise asks about the Argonaut can now at last, after many centuries, be answered in some fullness, although observation of the animal has been beset with numerous difficulties. It is a fact that the shell is *not* necessary to its life, and Lacaze-Duthier observed the animal recover the shells which had been taken from it. The shell when fully formed is in no organic connexion with the body of the

[1] *Historia animalium*, ix. 38; $622^b$ 5. [2] *Historia animalium*, iv. 2; $525^a$ 22.

[3] *Historia animalium*, ix. 38; $622^b$ 6.

[4] On the question of the authenticity of the ninth book of the *Historia animalium*, see H. Aubert and F. Wimmer, *Aristotelis Thierkunde*, Leipzig, 1868, p. 11.

animal, and its function is but to support and aerate the developing eggs; it has therefore been aptly compared to a perambulator. The animal does not willingly sink below the surface of the water, and if forced to do so will rise again, and it is indeed doubtful if it is able to sink at all by its own effort. The act of congress has not been seen in this species, though since the appearance of the work of Heinrich Müller, the male, which is much smaller than the female, has been recognized. (Plate XIII.)

Turning now to the description given of the sexual processes of cephalopods by the naturalist of antiquity we read that

'The Malacia such as the octopus, the sepia and the calamary, have sexual intercourse all in the same way; that is to say, they unite at the mouth by an interlacing of their tentacles. When, then, the octopus rests its so-called head against the ground and spreads abroad its tentacles, the other sex fits into the outspreading of these tentacles, and the two sexes then bring their suckers into mutual connexion. Some assert that the male has a kind of penis in one of his tentacles, the one in which are the largest suckers; and they further assert that the organ is tendinous in character, growing attached right up to the middle of the tentacle, and that the latter enables it to enter the nostril or funnel of the female.' [1]

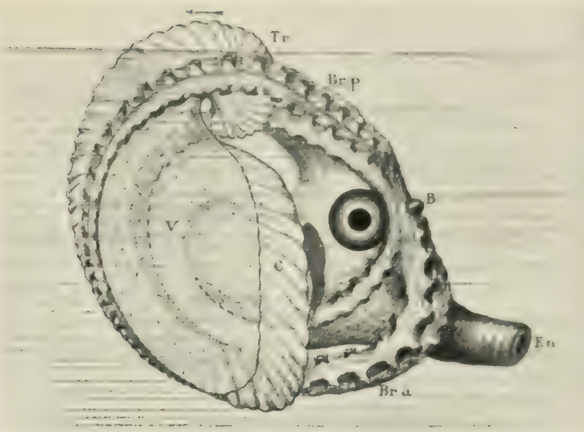
It is unfortunate that since he has given this accurate description Aristotle elsewhere contradicts it.

'In the cephalopods', he says, 'the same passage serves to void excrement and leads to the part like a uterus, for the male discharges the seminal fluid through this passage. And it is on the lower surface of the body where the mantle is open. . . . Hence the union of the male with the female takes place at this point. . . . But the insertion, in the case of the poulps, of the arm of the male into the funnel of the female, by which arm the fishermen say the male copulates with her, is only for the sake of attachment, and it is not an organ useful for generation, for it is outside the passage in the male and indeed outside the body of the male altogether.' [2]

We may now turn to the rediscovery in modern times of that peculiar sexual process of the octopods known as *hectocotylization* which explains Aristotle's description and even his self-contradiction concerning the cephalopods. The story may be given in the words of one of the most eminent investigators of these creatures, Richard Owen. The discussion concerning the nature of the process of fertilization arose chiefly in connexion with the octopod known as *Argonauta Argo*.

[1] *Historia animalium*, v. 6; 541$^b$ 1.

[2] *De generatione animalium*, i. 15; 720$^b$ 25.




Female argonaut from H. de Lacaze-Duthiers, *Archives de Zoologie expérimentale*, 1892.

The animal is seen in profile and the arrow shows the direction of movement. *B*, mouth with parrot-like beak. *Tr*, the nidamental shell projecting beyond the two specially developed shell-carrying arms *Br.p.* If the outline of the shell is followed it will be seen to project beyond the shell membrane *V*, also at *C*. *En*, the funnel projecting between the two anterior arms, *Br.a.*

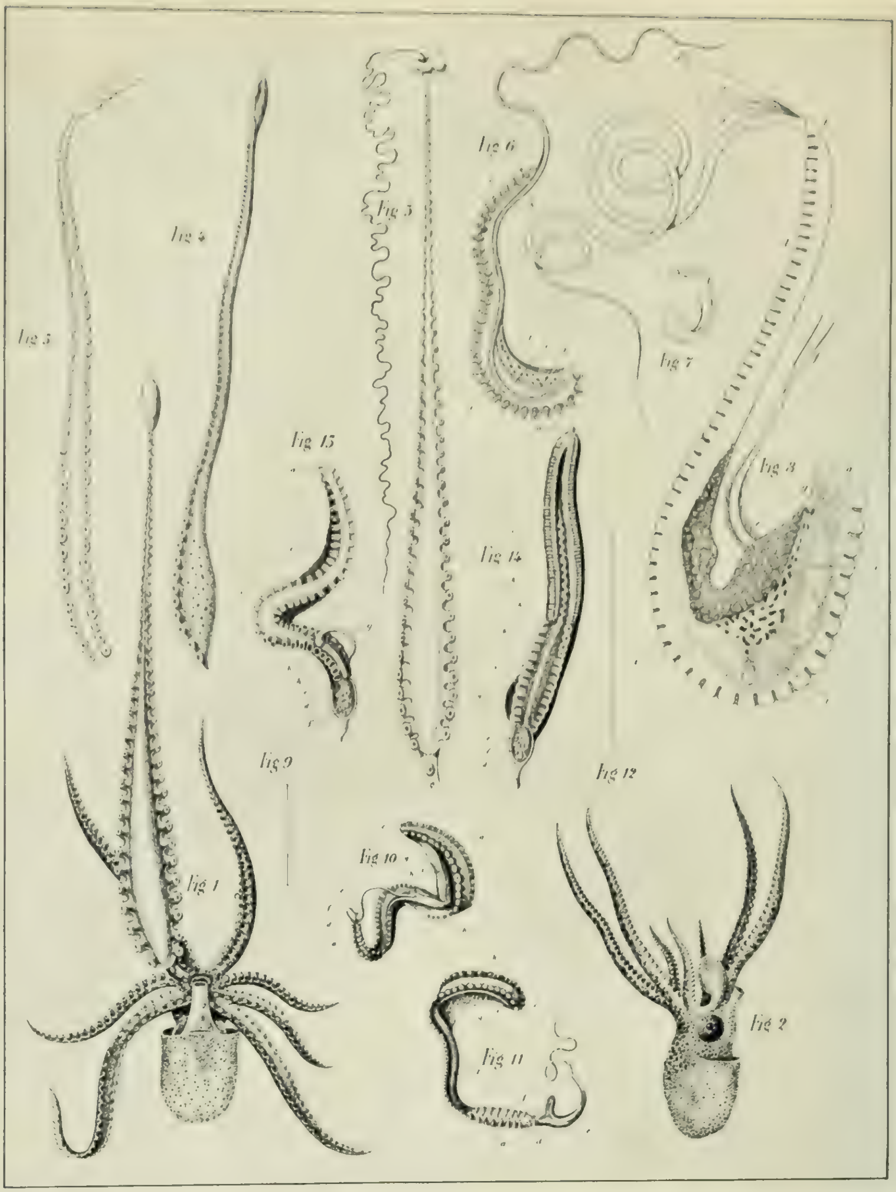


Male argonaut from Heinrich Müller, *Zeitschrift für wissenschaftliche Zoologie*, 1853.

1. The second and fourth arms of the left side are depressed to show the hectocotylus sac.

2. Hectocotylus unfolded showing how the sac is formed as a membrane developed at base of arm.

PLATE XIV




HECTOCOTYLIZATION

From J. B. Vérany, 1851; 1–3, *Octopus carenae* of Vérany; 1, Hectocotylus extended; 2, in sac; 3, removed from sac; 4–5, *Hectocotylus octopodis* of Cuvier, 1829; 6, *Tricho cephalus acetabularis* of Delle Chiaje, 1828; *lb* alimentary canal, *c* ovary, *d* pigmented membrane, *ef* suckers; 7-8, *Alleged male of Argonaut* from Costa, 1841; *ab* trunk, *cc* terminal appendix, *ef* tentacular cirrhus, *l* suckers, *ii* mucous sac, *d* membrane with special strands *xx*; 10–11, *Hectocotylus Argonautae* of Kölliker, 1849 (see text figs. 19-21); 13–14 Hectocotylus of *Tremoctopus violaceus* of Vérany after Kölliker, figured by the latter as a separate animal, *e* spermatic duct, *f* testis, *g* penis.

'The cumulative experience of numerous observers since 1839', says Owen, writing in 1855, 'had led to the conviction that the Argonauta with the expanded arms and shell was the female form of the species. The discovery of the male has been attended with difficulties. . . .

'Delle Chiaje first (1828) figured and described an organism which he found attached to the female Argonaut,[1] and which he believed to be a parasite, describing it under the name of *Trichocephalus acetabularis* (Plate XIV, item 6) on account of the number


FIG. 18. THE PAPER NAUTILUS

*Argonauta Argo.* From Belon's *Histoire naturelle des estranges poissons*, Paris, 1551. The animal is drawn as though using its arms as oars and its membrane as a sail.

of suckers with which it was beset. In the following year, Cuvier, having received a similar organism which Laurillard had detected in a cephalopod called *Octopus granulosus*, also believed it to be a parasitic worm for which he proposed the name of *Hectocotylus Octopodis*, assigning the name *Hectocotylus Argonautae* to the previously observed species.[2] (Plate XIV, items 4 and 5.) In 1842 Kölliker, having detected the same organism apparently parasitic on the female Argonaut, carefully scrutinized its structure and found that of the skin, with its complex pigment cells and that of the acetabula, identical with the same parts in the Argonaut. He detected, moreover, in a dilated hollow part of the organism a quantity of spermatozoa . . . and came to the bold conclusion

[1] Stefano delle Chiaje, *Memorie sulla storia e notomia degli animali senza vertebre del regno di Napoli*, 4 vols. and 2 atlases, Naples, 1828, vol. ii, Plate 16.

[2] Georges Cuvier, 'Mémoire sur un ver parasite d'un nouveau genre (*Hectocotylus octopodis*)', in the *Annales des sciences naturelles*, xviii, Paris, 1829, p. 147, plate 11.

that it was the long-sought-for male of the Argonaut, arrested in its development and subsisting practically parasitically on the female, like the diminutive males of the Rotifera, Epizoa, and Cirrhipedia. It may serve as a wholesome warning against entering upon

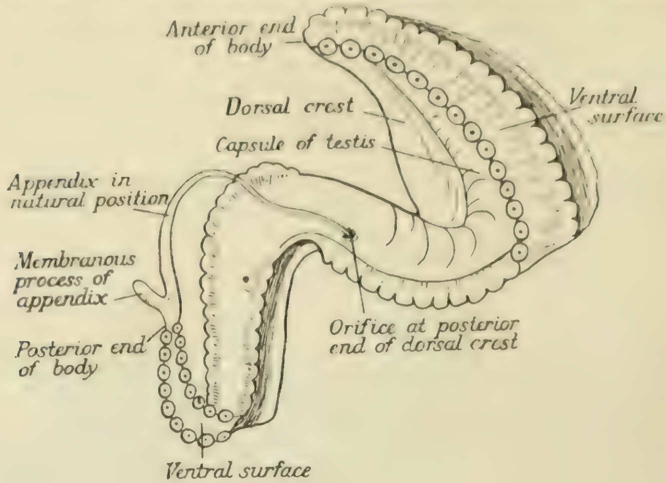



FIG. 19.

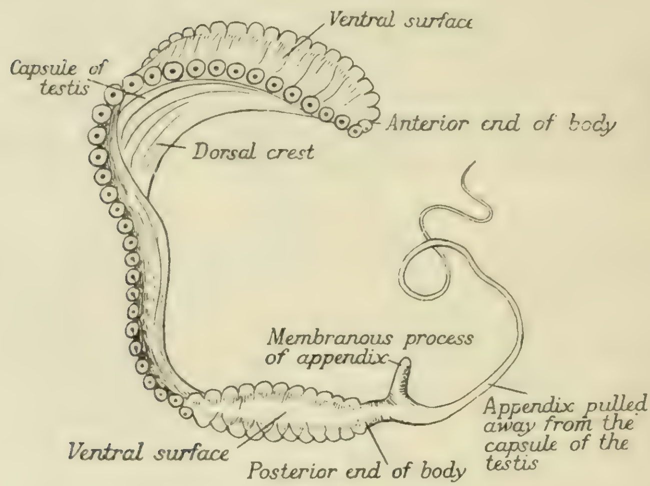



FIG. 20.

FIGS. 19, 20. DRAWING FROM KÖLLIKER OF THE 'MALE ARGONAUT'

a scrutiny of parts while prepossessed by a foregone conclusion to remember that the acute and usually accurate observer describes the digestive, circulatory, and respiratory organs of the same supposed independent individual male animal.' [1] (Figs. 19–21 and Plate XIV, items 10, 11, 13, and 14.)

[1] Albrecht von Kölliker, 'Hectocotylus Argonautae D. Ch. und Hectocotylus Tremoctopodis Köll., die Männchen von Argonauta Argo und Tremoctopus

'Vérany first had the good fortune to discover the Hectocotylus or presumed parasitic male Argonaut, forming one of the arms singularly modified and developed, of a little octopod which he figured under the name of *Octopus carenae* (Plate XIV, items 1–3).[1] Müller and others were not slow in demonstrating that this, or a similarly modified octopod, was really the male of the Argonauta [2] (Plate XIII).

'Certain species of the Octopod family thus have the male apparatus extended into one of the cephalic arms. . . . In *Octopus granulosus* and *Argonauta Argo* the spermatic duct is continued from the testis . . . into the base of the sexual arm and opens into a dilated reservoir at the termination of that singularly modified member. It is somewhat longer than the longest of the unmodified arms, and is much thicker. The acetabula are larger and more numerous, but retain the arrangement in a double row. . . .

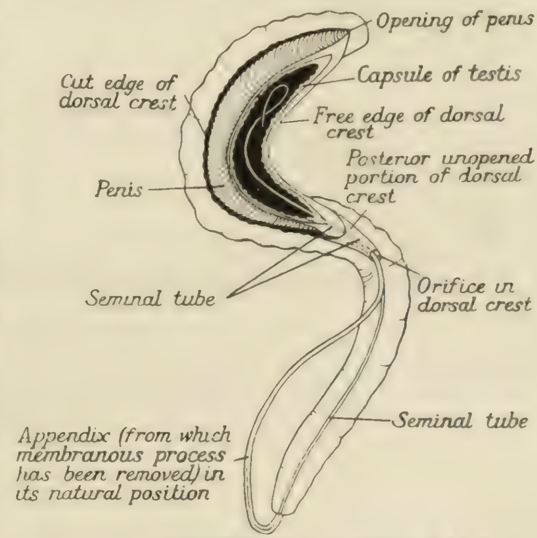



FIG. 21. DISSECTION OF THE 'MALE ARGONAUT'. After Kölliker.

'One presumes that the arms of the two sexes being interlaced, as described by Aristotle, the expanded receptacular end of the modified arm, with the spermatozoa, is introduced into the funnel of the female . . . and that . . . the modified arm is snapped off and left adhering to the female by the suckers where it long retains the power of motion. Such is the conclusion of the long mooted questions of the Argonaut, its shell, the use of the brachial membranes as 'sails', and the true sexual distinctions of the male and female.' [3]

violaceus D. Ch.', in the *Berichte von der zootomischen Anstalt in Würzburg für das Schuljahr* 1847–8, Leipzig, 1849, p. 67. This article is exceedingly rare, but there is a copy in the collection of T. H. Huxley at the Royal College of Science, London.

[1] J. B. Vérany, *Mollusques méditerranéens*, Genoa, 1851, and 'Mémoire sur les hectocotyles de quelques Céphalopodes' in the *Annales des sciences naturelles*, xvii, Paris, 1852.

[2] Heinrich Müller, 'Ueber das Männchen von Argonauta Argo und die Hectocotylen' in the *Zeitschrift für wissenschaftliche Zoologie*, vol. iv, Leipzig, 1853, p. 1.

[3] Richard Owen, *Lectures on the Anatomy and Physiology of the Invertebrates*, 2nd edition, London, 1855, pp. 628 ff.

It remains only to add that the actual process of congress of an octopus, though not of *Argonauta* itself, has been watched by Racovitza (Fig. 22) and the hectocotylized arm of the male has been observed to be inserted through the mantle and into the

FIG. 22. CONGRESS OF OCTOPUS VULGARIS
as observed in a tank by É. Racovitza, *Archives de Zoologie expérimentale*, Série ii, Paris. 1894. The male is seen to the right. He has fixed himself by the bases of his arms to the glass of the aquarium and has introduced the extremity of the hectocotylized arm into the pallial cavity of the female to the left.

funnel of the female.[1] There is still a gap in our knowledge however, for the passage of the fertilizing elements from the testis into the arm of the male has not yet been observed.

### (*d*) *The Habits of Animals*

#### i. *The Frog-fish and Torpedo*

Aristotle is perhaps at his best and happiest when describing the habits of living animals that he has himself observed. Among his most pleasing accounts are those of the fishing-frog and torpedo. In these creatures he did not fail to notice the displacement of the fins associated with the depressed form of the body.

' In the Torpedo and the Fishing-frog,' he says, ' the breadth of the anterior part of the body is not so great as to render locomotion by fins impossible, but in consequence of it the upper pair [*pectorals*] are placed further back and the under pair [*ventrals*] are placed close to the head, while to compensate for this advancement they are reduced in size so as to be smaller than the upper ones. In the Torpedo the two upper fins [pectorals] are placed in the tail, and the fish uses the broad expansion of its

[1] Émile Racovitza, ' Notes de biologie ' in the *Archives de zoologie expérimentale*, series 3, vol. ii, Paris, 1894, p. 25.

body to supply their place, each lateral half of its circumference serving the office of a fin.' [1]

'In marine creatures one may observe many ingenious devices adapted to the circumstances of their lives. For the account commonly given of the frog-fish or angler is quite true; as is also that of the torpedo. The frog-fish has a set of filaments that project in front of its eyes; they are long and thin, like hairs, and are round at the tips; they lie on either side, and are used as baits.[2] . . . The little creatures on which this fish feeds swim up to the filaments, taking them for bits of seaweed such as they feed upon.[3] Accordingly, when the animal stirs himself up a place where there is plenty of sand and mud and conceals himself therein, it raises the filaments, and when the little fish strike against them the frog-fish draws them in underneath into its mouth. The torpedo narcotizes the creatures that it wants to catch, overpowering them by the force of shock that is resident in its body, and feeds upon them; it also hides in the sand and mud, and catches all the creatures that swim in its way and come under its narcotizing influence. This phenomenon has been actually observed in operation. . . . That the creatures get their living by this means is obvious from the fact that, whereas they are peculiarly inactive, they are often caught with mullets, the swiftest of fishes, in their interior. Furthermore, the frog-fish is unusually thin when he is caught after losing the tips of his filaments, while the torpedo fish is known to cause a numbness even in human beings.' [4]

The Fishing-frog is well known in northern waters and several of the early naturalists give recognizable figures of it (Fig. 23). It is remarkable how in most of them a curious error has crept in as regards the structure of the fins. The pectoral limbs of the Lophiidae are, in fact, specially developed for crawling about on the sea floor. In the older pictures, however, the fins are usually represented as divided into digits, a mistake perpetuated in the careful paintings of dissections of this creature that have been left by Edward Tyson (1650–1708) [5] (Plate XII).

The peculiar action of the Torpedo described by Aristotle has been the subject of a number of classical investigations and has been brought into line with widely distributed phenomena. It is characteristic of muscle substance that at the moment of contraction it produces an electric disturbance. In ordinary

[1] *De partibus animalium*, iv. 13; $696^a$ 26.

[2] *Historia animalium*, ix. 37; $620^b$ 10.

[3] This passage only doubtfully refers to the fishing-frog. We have transposed it from ix. 37; $620^b$ 30.

[4] *Historia animalium*, ix. 37; $620^b$ 15.

[5] Drawings of these dissections are found in MSS. of Tyson at the Royal College of Physicians of London, and at the British Museum.

muscular tissue this can only be detected by means of delicate instruments, but in the Torpedo-fish there are two tracts of musculature which show a reduction of the function of contraction accompanied by a great increase in the power of producing electric disturbance. These electric organs are two large kidney-shaped structures occupying the greater portion of the forepart of the animal and are controlled by a special set of nerves which have their origin in a characteristically developed lobe of the brain (Fig. 24).

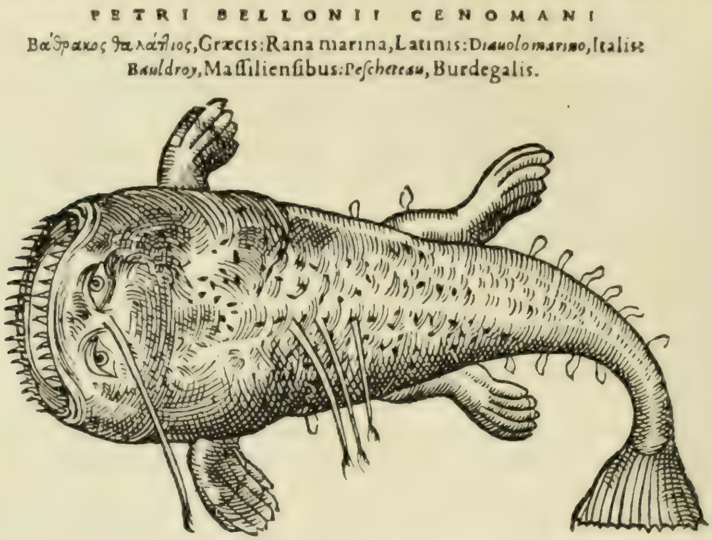



FIG. 23. THE FROG-FISH
From Pierre Belon's *De aquatilibus*, Paris, 1553.

Aristotle was by no means alone among the ancients in his knowledge of the electric action of the torpedo. The animal was commonly eaten and is referred to as suitable for invalids in several of the Hippocratic treatises (fifth–fourth cent. B.C.).[1] Torpedo and fishing-frog were both known to Pliny (*c.* A. D. 23–79), and it is worth repeating his account of these fishes, copied evidently from Aristotle, in Philemon Holland's inimitable translation:

'I marvel', he writes, 'at them who are of opinion that fishes and beasts in the water have no sence. Why, the very Crampe-fish Tarped (i. e. *Torpedo*) knoweth her oune force and power, and being herselfe not benummed, is able to astonish others. She lieth hidden ouer head and ears within the mud unseene, readie to catch those fishes, which as they swim ouer her, bee taken with

[1] In the *De victus ratione*, a work in part older than Hippocrates, in the spurious but ancient *De internis affectionibus*, and in the equally ancient and spurious *De mulierum morbis*.

a nummednesse, as if they were dead. Also the fish called the sea Frog, Diable de Mer, (and of others, the sea Fisher) is as craftie everie whit as the other: It puddereth in the mud, and troubleth the water, that it might not bee seene: and when the

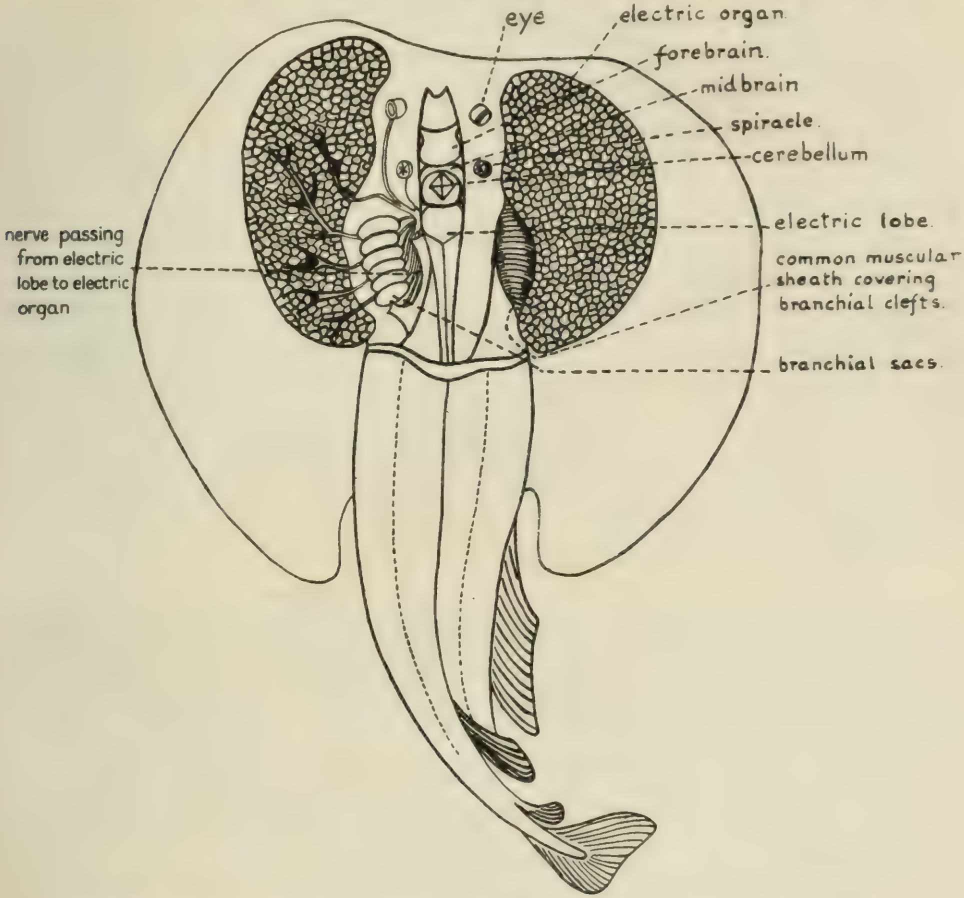



FIG. 24. THE ELECTRIC ORGAN OF THE TORPEDO

little seely fishes come skipping about her, then she puts out her little hornes or Barbils which shee hath bearing forth under her eies, and by little and little tilleth and tolleth them so neere that she can easily seaze upon them.' [1]

The torpedo must have been somewhat extensively experimented upon in antiquity. Galen (A. D. 131–201) knew of its power, which he compared to the magnetic effect of a lodestone;[2] he noted that it produced numbness or anaesthesia of parts that

[1] *The Historie of the World, commonly called, the Naturall Historie of* C. Plinius Secundus, *Translated into English by Philemon Holland Doctor in Physicke*, 2 vols., London, 1601. The quotation is from book ix, chapter 42, of Holland's notation.

[2] Galen, *De locis affectis*, Lib. vi. Kühn viii. 421.

it touched [1] and he therefore recommended the application of the living fish to the head for headache.[2] Similar remedies are referred to by Scribonius Largus (A. D. 47), Marcellus Empiricus of Bordeaux (fifth century), and Aetius of Amida (sixth century).

The Torpedo and its powers have been known continuously from Greek times, but many centuries went by without any elucidation of its structure. The first to suggest the true nature of the shock of electric fish was Muschenbroeck in 1762, who compared their shocks to those of a Leyden jar.[3] A series of experiments on the Torpedo was undertaken by Walsh in 1772,[4] and by Ingenhousz in 1775,[5] and about the same time the structure of the electric organ was described by John Hunter.[6] The special electric lobe of the brain and its homologies and those of the nerves that arose from it were not completely elucidated until the work of Letheby in 1843.[7]

### ii. *Bees*

Aristotle's account of bees is remarkable for its extent. He has given more attention to these than to any other group of insects. Yet it must be confessed that although his account contains many accurate and striking observations, it also contains numerous errors and is obviously largely drawn from hearsay and from secondhand accounts. We have omitted many erroneous elements, and have put together some of his best passages on these insects.

Of the structure of the bee Aristotle tells us very little, nor does that little contain any evidence of close observance of the actual parts. We read that 'some insects have the part which serves as tongue inside the mouth as with ants . . . while in others

[1] Galen, *De locis affectis*, Lib. ii, Kühn viii. 72, and *De symptomatum causis*, Lib. i, cap. 5, Kühn, vii. 109.

[2] Galen, *De simplicium medicamentorum temperamentis ac facultatibus*, Lib. xi, cap. 11, Kühn, xii. 365.

[3] Pieter van Muschenbroeck, *Introductio ad philosophiam naturalem*, Leyden, 1762.

[4] J. Walsh, 'On the Electric Property of the Torpedo', *Philosophical Transactions*, London, 1772.

[5] J. Ingenhousz, 'Experiments on the Torpedo', *Philosophical Transactions*, London, 1775.

[6] John Hunter, 'Anatomical Observations on the Torpedo', *Philosophical Transactions*, July 1, 1773.

[7] H. Letheby, 'An account of a second *Gymnotus Electricus*, together with a description of the electrical phenomena and the anatomy of the Torpedo', *Proceedings of the London Electrical Society for 1843*, p. 512, London, 1843.

MS Bodley 130 fo. 37r. Written at St Albans 1120


MS Sloane 1975 fo. 15r. Written in England
Early XIIIth Cent.

it is placed externally. In this latter case it resembles a sting, and is hollow and spongy, so as to serve at one and the same time for tasting and for the taking up of nutriment. This is plainly to be seen in flies and bees.' [1] Thus in these animals, as he tells us, the proboscis protrudes from the mouth,[2] and he generalized correctly that in insects the mouth parts are either biting or sucking. Beyond this stage Western biology hardly advanced until the time of Mouffet (1553–1604), who prepared beautiful and accurate paintings of a large number of insects. Unfortunately his best figures of bees have greatly deteriorated. We reproduce, however, his drawing of the hornet (Plate LV).[3] Definite improvement of Mouffet's work was made by the members of the first Academy of the Lynx, a small body that came into existence in 1603 and fell to pieces with the death of their president, Federigo Cesi, in 1628. For the researches of the Academy the microscope was used, and as the instrument was hardly available until 1611 (vide Article No. 10 of this volume), the period during which their microscopic work was done is further narrowed.[4] The figure that we reproduce is the earliest drawing known for which the microscope was certainly used (Fig. 25). It was probably made about 1625.

Aristotle's description of the habits of bees is much more entertaining and complete than his account of the structure of these creatures. It is impossible, however, to give more than a fraction of what he has to say on the subject.

'There are nine varieties [of bee-like creatures] of which six are gregarious—the bee, the king-bee, the drone-bee, the annual wasp, . . . the anthrene [or hornet], and the tenthredo [or ground-wasp]; three are solitary—the smaller siren, of a dun colour, the larger siren, black and speckled, and the third, the largest of all, that is called the humble-bee.[5] . . . The little bees are more industrious than the big ones: their wings are battered; their colour is black, and they have a burnt-up aspect. Gaudy and showy bees, like gaudy and showy women, are good for nothing.' [6]

[1] *De partibus animalium*, ii. 17; $661^a$ 15.

[2] *De partibus animalium*, iv. 5; $678^b$ 15.

[3] The figures in the printed work of Mouffet are much inferior to the drawings in his MS. See his *Insectorum sive minimorum animalium Theatrum*, London, 1634, English translation in E. Topsell's *History of four-footed beasts and serpents*, London, 1658, and compare with Brit. Mus. MS. Sloane 4014 (Plate LV).

[4] Early microscopes and microscopic observations are described in two papers by Charles Singer, 'Notes on the Early History of Microscopy,' *Proc. Roy. Soc. of Med.*, London, 1914, vol. vii (sect. of Hist. of Med.), p. 247, and 'The Dawn of Microscopical Discovery,' *Journal Roy. Microscopical Soc.*, London, 1915, p. 317.

[5] *Historia animalium*, ix. 39; $623^b$ 7. [6] *Historia animalium*, ix. 40; $627^a$ 12.

'Ants never go a-hunting, but gather up what is ready to hand; the spider makes nothing, and lays up no store, but simply goes a-hunting for its food; while the bee . . . does not go a-hunting, but constructs its food out of gathered material and stores it away, for honey is the bee's food. . . . They have also another food which is called bee-bread; this is scarcer than honey and has a sweet, fig-like taste; this they carry, as they do the wax, on their legs.

'Very remarkable diversity is observed in their methods of working and their general habits. When the hive has been delivered to them clean and empty, they build their waxen cells, bringing in the juice of all kinds of flowers and the "tears" or exuding sap of trees. . . . With this material they besmear the ground-work, to provide against attacks of other creatures; . . . they also with the same material narrow by side-building the entrances to the hive if they are too wide. . . . They begin building the combs downwards from the top of the hive, and go down and down building many combs connected together until they reach the bottom. The cells, both those for the honey and those also for the grubs, are double-doored; for two cells are ranged about a single base, one pointing one way and one the other, after the manner of a double goblet. The cells that lie at the commencement of the combs and are attached to the hives, to the extent of two or three concentric circular rows, are small and devoid of honey; the cells that are well filled with honey are most thoroughly luted with wax.[1] The ordinary bee is generated in the cells of the comb, but the ruler-bee in cells down below attached to the comb, suspended from it, apart from the rest, six or seven in number, and growing in a way quite different from the mode of growth of the ordinary brood.[2]

'The drones, as a rule, keep inside the hive; when they go out of doors, they soar up in the air in a stream, whirling round and round in a kind of gymnastic exercise: when this is over, they come inside the hive and feed to repletion ravenously.[3] . . . The kings are never themselves seen outside the hive except with a swarm in flight: during which time all the other bees cluster around them.[4] . . . These rulers have the abdomen or part below the waist half as large again, and they are called by some the "mothers" from an idea that they bear or generate the bees; and, as a proof of this theory of their motherhood, they declare that the brood of the drones appears even when there is no ruler-bee in the hive, but that the bees do not appear in his absence. Others, again, assert that these insects copulate, and the drones are male and the bees female.[5]

'Bees scramble up the stalks of flowers and rapidly gather the beeswax with their front legs; the front legs wipe it off on to the middle legs, and these pass it on to the hollow curves of the hind legs; when thus laden, they fly away home, and one may see plainly that their load is a heavy one. On each expedition

[1] *Historia animalium*, ix. 39; 623ª 13. [2] *Historia animalium*, v. 22; 553ᵇ 2.

[3] *Historia animalium*, ix. 40; 624ª 22. [4] *Historia animalium*, ix. 40; 625ᵇ 7.

[5] *Historia animalium*, v. 21; 553ª 27.

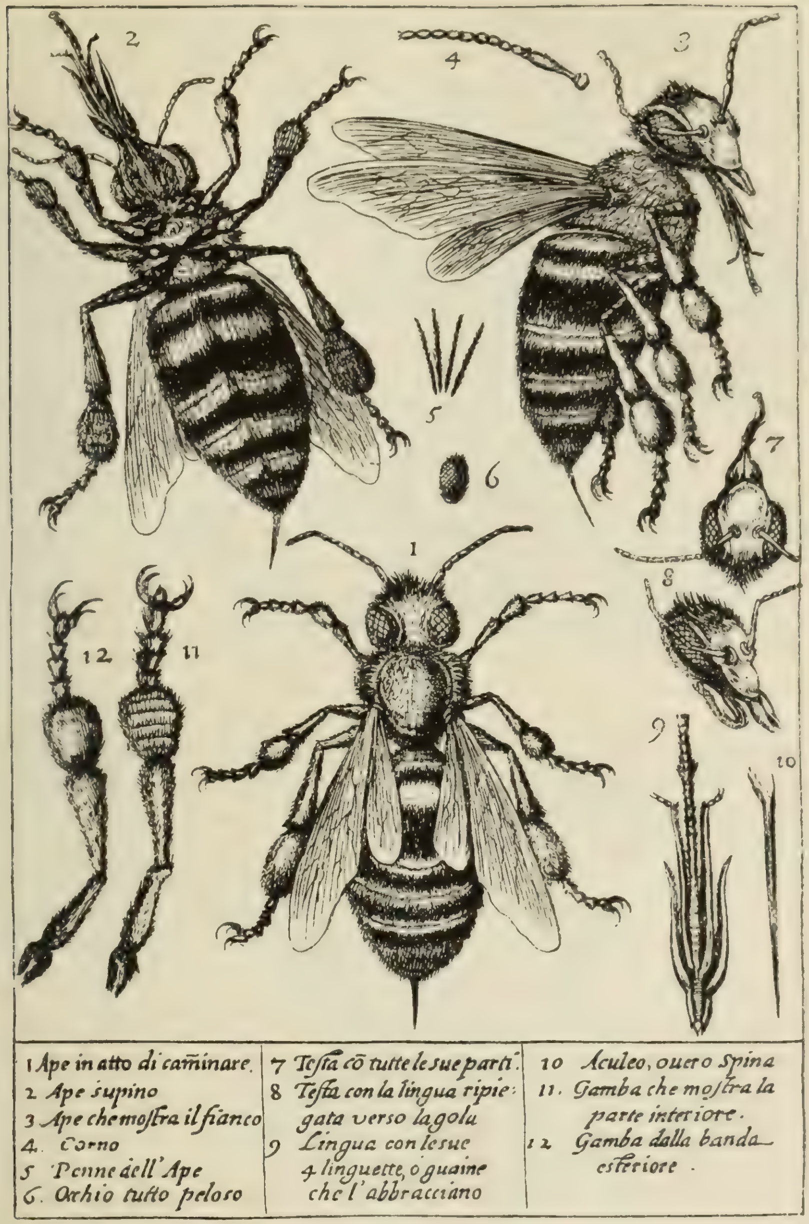



Fig. 25. ENLARGED FIGURES OF THE BEE

From Francesco Stelluti's *Persio tradotto*, Rome, 1630. The plate is the product of the Accademia dei Lincei. It was based on the work of Cesi (d. 1628), drawn by Fontana (probably about 1625), and contains observations by Johannes Faber and Francesco Stelluti. It is probably the earliest printed figure drawn with the aid of the microscope.

the bee does not fly from a flower of one kind to a flower of another, but flies from one violet, say, to another violet, and never meddles with another flower until it has got back to the hive; on reaching the hive they throw off their load, and each bee on his return is accompanied by three or four companions. One cannot well tell what is the substance they gather nor the exact process of their work.[1] . . . Bees feed on thyme: and the white thyme is better than the red.[2] . . . Bees seem to take a pleasure in listening to a rattling noise: and consequently men say that they can muster them into a hive by rattling with crockery or stones.[3] . . . Whenever the working-bees kill an enemy they try to do so out of doors.[4] Bees that die are removed from the hive, and in every way the creature is remarkable for its cleanly habits; in point of fact, they often fly away to a distance to void their excrement because it is malodorous; and, as has been said, they are annoyed by all bad smells and by the scent of perfumes, so much so that they sting people that use perfumes.[5]

'When the flight of a swarm is imminent, a monotonous and quite peculiar sound made by all the bees is heard for several days, and for two or three days in advance a few bees are seen flying round the hive; it has never as yet been ascertained, owing to the difficulty of the observation, whether or no the king is among these. When they have swarmed, they fly away and separate off to each of the kings; if a small swarm happens to settle near to a large one, it will shift to join this large one, and if the king whom they have abandoned follows them, they put him to death. So much for the quitting of the hive and the swarm-flight. Separate detachments of bees are told off for diverse operations; that is, some carry flower-produce, others carry water, others smooth and arrange the combs. A bee carries water when it is rearing grubs. No bee ever settles on the flesh of any creature, or ever eats animal food. . . . When the grubs are grown, the bees put food beside them and cover them with a coating of wax; and, as soon as the grub is strong enough, he of his own accord breaks the lid and comes out. . . .

'When the bee-masters take out the combs, they leave enough food behind for winter use; if it be sufficient in quantity, the occupants of the hive will survive; if it be insufficient, then, if the weather be rough, they die on the spot, but if it be fair, they fly away and desert the hive. Away from the hive they attack neither their own species nor any other creature, but in the close proximity of the hive they kill whatever they get hold of. Bees that sting die from their inability to extract the sting without at the same time extracting their intestines. True, they often recover, if the person stung takes the trouble to press the sting out; but once it loses its sting the bee must die.[6]

The diseases that chiefly attack prosperous hives are (*a*) first of

[1] *Historia animalium*, ix. 40; 624ᵃ 35. [2] *Historia animalium*, ix. 40; 626ᵃ 20.
[3] *Historia animalium*, ix. 40; 626ᵃ 15. [4] *Historia animalium*, ix. 40; 625ᵃ 31.
[5] *Historia animalium*, ix. 40; 626ᵃ 23. [6] *Historia animalium*, ix. 40; 625ᵇ 6.

Plate XVI   Cotton Vitellius C.III   Anglo-Saxon Work of about 1000

fo. 21v   Wagbraede = Waybroad = Meadow Plantain


fo. 23v   Hennebelle = Henbane = *Hyoscyamus reticulatus*, a Mediterranean species

all the *clerus*—this consists in a growth of little worms on the floor, from which, as they develop, a kind of cobweb grows over the entire hive, and the combs decay.[1] . . . Bees brood over the combs and so mature them; if they fail to do so, the combs are said to go bad and to get covered with a sort of spider's web. . . . When the combs keep settling down, the bees restore the level surface, and put props underneath the combs to give themselves free passage-room; for if such free passage be lacking they cannot brood, and the cobwebs come on.[2] . . . (*b*) There is another insect resembling the moth, called by some the *pyraustes*, that flies about a lighted candle: this creature engenders a brood full of fine down. It is never stung by a bee, and can only be got out of a hive by fumigation. (*c*) A caterpillar also is engendered in hives, of a species nicknamed the *teredo*, or "borer", with which creature the bee never interferes.[3] . . . (*d*) Another diseased condition is indicated in a lassitude on the part of the bees and in malodorousness of the hive.'[4]

The account of the diseases of hives described by Aristotle is paraphrased by Pliny.[5] These diseases can be identified with comparative confidence.

(*a*) The *Clerus* is held to be the *Trichodes apiarius*, a red and blue beetle which is known to destroy the larvae of the honey bee. When honeycomb is destroyed from this or any other cause a mould is liable to grow upon it. It is possible that Aristotle was confusing this growth with the cocoons described under the next heading.[6]

(*b*) The *moths that inhabit hives* are *Galleriidae*, a small group, many of which, *Galleria mellonella* especially, live in and consume bee hives. The spider's web described above is probably, in part, the cocoon of this species.

(*c*) The *caterpillars called Teredo* are the larvae of species of the *Galleriidae*, some of which have the habit of spinning their cocoons into a mass which is perhaps described by Aristotle as the 'cobweb' and also as the 'brood full of fine down'.

(*d*) This is some form of *foul brood*, a condition first described in modern times by Schirach in 1769,[7] and more minutely and carefully in the island of Syra by Della Rocca[8] towards the end of the eighteenth century. The actual nature of the disease—in the causation of which a number of spore-bearing organisms are

[1] *Historia animalium*, ix. 40; 626$^b$ 16. [2] *Historia animalium*, ix. 40; 625$^a$ 6.

[3] *Historia animalium*, viii. 27; 605$^b$ 12. [4] *Historia animalium*, ix. 40; 626$^b$ 18.

[5] Pliny, xi. 20.

[6] C. J. Sundevall, *Die Thierarten des Aristoteles*. Stockholm, 1863.

[7] A. G. Schirach, *Der Sächsische Bienenmeister, oder kurze Anweisung für den Landmann zur Bienenzucht, nebst beygefügtem Bienenkalender*, Löbau, 1769.

[8] L'abbé Della Rocca, *Traité complet sur les Abeilles avec une méthode nouvelle de les gouverner, telle qu'elle se pratique à Syra*, Paris, 2 vols., 1790.

involved—has been more fully cleared up only in quite modern times, beginning with the work of Ferdinand Cohn (1828–98).

Despite the figures of Mouffet prepared late in the sixteenth century,[1] and of the Academy of the Lynx early in the seventeenth,[2] the knowledge of the various forms of bee advanced but slowly. The male, female, and neuter were not adequately distinguished for what they were until the work of Goedart and de Mey in 1662.[3] The knowledge of the habits of these creatures was even more behindhand, and although some advance was made by Charles Butler (died 1647) in 1609,[4] yet Rusden in 1689[5] was still describing the queen bee as a *king* in the language of Aristotle. The whole subject had, however, by that time, been put on a firmer basis by the microscopical researches of Swammerdam (between 1662 and 1675)[6] and Malpighi[7] (about 1680).[8]

## V. The General Course of Botanical Knowledge

### (*a*) *Botany among the Greeks*

The history of Botany is more fortunate than that of its companion branch of Biology in that the material exists for telling it as an almost continuous story. Thus for the observation of plants our own age is linked with, and not separated from, that of the Greeks. Much of the material, especially for the period from the sixth to the twelfth century, is difficult of access, and this we have sought to present here in greater detail than its quality would otherwise demand.

Among uncivilized peoples the knowledge of plants is by no means confined to their culinary uses. Even the most primitive races have also a herb-lore which instructs them what plants to use and how to use them for the treatment of disease. Much effort has been expended in attempting to demonstrate the

[1] British Museum MS. Sloane 4014, written before 1589.

[2] Francesco Stelluti, *Persio tradotto*, Rome, 1630, illustrates this work. The more important *Apiarium* of Federigo Cesi, Rome, 1625, is excessively rare, and I believe there is no copy in this country.

[3] Jean Goedart, *Metamorphosis et historia naturalis insectorum cum commentario Iohannis de Mey*, Middelburg (1662–7).

[4] Charles Butler, *The Feminine Monarchie or a Treatise concerning Bees and the due ordering of Bees*, Oxford, 1609.

[5] Moses Rusden, *A Further Discovery of Bees*, London, 1689.

[6] Jan Swammerdam, *Bybel van de Natuur*, Leyden, 1737.

[7] Marcello Malpighi, *Opera Omnia*, London, 1686.

[8] A useful article on the knowledge of bees displayed by the Aristotelian writing is by J. Klek and L. Armbruster, 'Die Bienenkunde des Aristoteles und seiner Zeit'. *Archiv für Bienenkunde*, i, Abt. 6, Freiburg im Breisgau, 1919.

therapeutic value of such drugs, but it would appear that folk herb-lore is no more rational than other departments of folk medicine and the majority of the ingredients of all pharmacopoeias, save perhaps the most modern, are in fact without appreciable physiological action and certainly are no cures for the conditions for which they are given.

The recipients of the herb-lore traditions among the Greeks were the *rhizotomists*. These, as a class, were ignorant men, corresponding in a measure to our herbalists, and they occupied themselves with gathering herbs, sometimes for the use of the physicians, sometimes in order that they might themselves usurp the functions of the physicians. They were superstitious and practised a complex ritual in obtaining their drugs. Fragments of this ritual have survived,[1] and we can detect in it ceremonies still closely followed by European peasantry engaged in similar practices. Rhizotomists were often of evil reputation, and Sophocles, who was contemporary with Hippocrates, wrote a play, now lost, entitled 'The Rhizotomists', the word having been used by him as almost equivalent to *poisonmongers*. The profession and to some extent the tradition of the rhizotomists extended from Greek into mediaeval and even modern times, and they and their work are not infrequently illustrated in the manuscripts (Fig. 26).

We learn little of the botanical knowledge of the sixth and fifth centuries from the works that make up the Hippocratic collection. About three hundred plants are mentioned as of value for medicinal purposes, and this implies considerable botanical knowledge, but we can glean almost nothing of the plants themselves from the Hippocratic writings. With the fourth century botany as a separate study comes into full view, and there is evidence that the subject may have been systematically studied at the Academy even before it was taken up by the Lyceum. From the latter we possess documents of first-class importance in the works of Theophrastus, the so-called 'father of botany', whose achievements are considered in greater detail below (p. 79).

But Theophrastus is the father of botany only in the sense that he is the first botanist whose writings have come down to us. There is nothing primitive about his work, nothing to suggest

[1] e. g. in Theophrastus, *Historia plantarum*, ix. 8. Other instances can be found in Pliny and have been collected by J. J. Mooney in his *Hosidius Geta's Tragedy 'Medea'*, Birmingham, 1919. Yet further material from both Greek and Latin sources may be culled from A. Abt, *Die Apologie des Apuleius von Madaura und die antike Zauberei*, Giessen, 1908.

that he is treading paths that none have trod before. On the contrary he is a thoroughly sophisticated writer, evidently the product of generations of thought and even of research. We know, moreover, that his work was preceded by a treatise on plants by Aristotle which is now lost, though fragments of it have perhaps survived.[1] But the writings of Theophrastus are specially valuable as presenting, after Hippocrates, the first substantially complete works of Greek observational science extant. Viewed in relation to the completeness and ordered sequence of the *History of Plants*, the biological works of Aristotle, in the form in which they have reached us, seem mere disarranged note-books. Like the works of Aristotle, those of Theophrastus are scientific compositions; that is to say, they are written, in large part at least, for the purpose of describing nature and not for the direct applicability of such knowledge to the needs and amenities of life. In this respect they stand almost alone among Greek botanical works, for the rest of Greek botanical history has to be pieced together mainly from poetical and pharmaceutical writings.

From the second century B. C. we have in Greek the *Alexipharmaca* and the *Theriaca* of Nicander, two works on poisons and their antidotes which deal with a number of plants from the point of view of their special topics and not from the scientific aspect. The *De re rustica* of Cato the Censor is contemporary with these works and is even more devoid of the Greek scientific spirit. For the first century B. C. we are better provided, for we have the literary works of Virgil and the agricultural treatises of Varro and Columella, supplemented by what can be gleaned from the valuable medical compendium of Celsus.

The first century of the Christian era is richer in important botanical works than any period from the fourth century B. C. until the sixteenth century. In Pliny's *Natural History* we have a collection of current views on the nature, origin, uses, and treatment of plants such as we might expect from a very intelligent, industrious, and honest member of the landed class who was devoid of critical or special scientific skill. More valuable and ranking second only to Theophrastus in importance is the treatise

[1] Put together by F. Wimmer, *Phytologiae Aristotelicae Fragmenta*, Breslau, 1838 (unfinished). The spurious *De Plantis* of Aristotle is attributed to Nicholas of Damascus by E. H. F. Meyer, *Nicolai Damasceni de plantis . . . Aristoteli vulgo adscripti ex Isaaci ben Honaici versione Arabica Latine vertit Alfredus*, Leipzig, 1841. It probably contains elements in the Aristotelian tradition. See note, p. 13.

Fig. 26. This drawing is traced from a facsimile on Plate xxii of the Atlas to Piero Giacosa's *Magistri Salernitani*, Turin, 1901. The MS. from which Giacosa's facsimile was taken has since perished. It bore the arms of Savoy and was work of the fifteenth century. The figure represents herbalists at work on a mountain, the slopes of which are covered with a variety of plants. One of the herbalists is climbing an oak tree to secure the mistletoe growing on it. The other is digging up plants and placing them in a receptacle.

on *Materia Medica* of Dioscorides. It consists of a series of careful descriptions of plants, and of their uses in medicine, arranged, it is true, almost without reference to the nature of the plants themselves, but quite invaluable for its terse and striking descriptions which often include habits and habitats. Its history has shown it to be the most influential botanical treatise ever penned.

After Dioscorides Greek botany declines. The monumental intellect of Galen in the second century of the Christian era hardly applied itself to plants, and his pharmacopoeia, copious though it is, gives but a scant glimpse of contemporary botany. Such Greek botanical writers as followed were little but copiers of Galen and Dioscorides, and the history of Greek botany is from now on a story of the continued disintegration of knowledge.

But, retracing our steps, there yet remains for consideration a writer of the first century B. C. the full importance of whom has only recently been realized. There is evidence that Crateuas, the most intelligent and instructed of the rhizotomists who lived during this century, occupied himself not only in collecting but also in drawing plants. He is thus the father of the very important botanical department of plant illustration.[1] Crateuas was the attendant of Mithridates VI Eupator, but was also an author, and though his works have perished, fragments have survived in a Vienna codex.[2] This manuscript, to which the name of Julia Anicia is attached, was prepared in Constantinople a little before the year 512 as a wedding gift to that lady, the daughter of Flavius Anicius Olybrius, Emperor of the West, and it is not improbable that many of the figures in it are copies of Crateuas's own drawings or of pictures prepared under his supervision in the first century B. C. He is perhaps represented at work in one of the more damaged miniatures of this great manuscript (Fig. 27), and we have what are perhaps portraits of Dioscorides himself in this and other miniatures of the same volume (Fig. 28).[3]

[1] Pliny, *Hist. Nat.* xxv. 4.

[2] Max Wellmann, 'Krateuas', *Abh. der kgl. Gesellschaft der Wissenschaften zu Göttingen, philologisch-historische Abt.*, Neue Folge, Bd. ii, No. 1, Berlin, 1897; and M. Wellmann, 'Das älteste Kräuterbuch der Griechen', in *Festgabe für Franz Susemihl*, Leipzig, 1898.

[3] These miniatures are represented in their damaged state in Rendel Harris, *The Ascent of Olympus*, Manchester, 1917. The entire MS. has been reproduced in two luxurious but excessively inconvenient elephant folios by J. de Karabacek, Leyden, 1906. The original work has been removed from Vienna to St. Mark's Library at Venice.

Thus even before the Julia Anicia MS., botanical illustration had had a long history, and surviving fragments of a herbal on papyrus of the second century [1] show that a strong tendency to diagrammatize the forms of plants had already set in among the


FIG. 27. Restored from the Julia Anicia MS., fo. 5 v. about A.D. 512. Dioscorides writes while Intelligence (Ἐπίνοια) holds the mandrake for the artist, Crateuas (?), to copy.

Greeks. But the tradition represented by the Julia Anicia MS. was that which for many centuries largely controlled the whole art of plant illustration in Greek-speaking lands. The history of this manuscript is thus itself the history of an important line of botanical knowledge during the Middle Ages. The Julia Anicia MS. is not exactly a text of Dioscorides but is a composite docu-

[1] J. de M. Johnson, 'A botanical Papyrus with Illustrations' in the *Archiv f. Gesch. d. Naturwissenschaften und der Technik*, iv, p. 403, Leipzig, 1913.

ment of which the nature and origin is represented in the diagram opposite (Fig. 29).

Even in the seventh century the sources of the Julia Anicia figures were still being imitated with considerable accuracy and


FIG. 28. Restored from the Julia Anicia MS., fo. 4 v. about A.D. 512. Discovery (Εὕρεσις) presents a mandrake to the physician Dioscorides. The mandrake is still tethered to the hound whose life is sacrificed to obtain it.

skill,[1] but as the centuries went by and the figures were copied and recopied without reference to the original they moved further and further from the facts until at last they remained as mere diagrams. By this time the gift of naturalistic representation had left the Greeks, but traces of some power to imitate classical models of

[1] As in the so-called Codex Neapolitanus of the seventh century at St. Mark's, Venice. A facsimile of a page of this work has been reproduced by the New Palaeographical Society, ii, Plate 45. Copies of the Julia Anicia itself, of which several exist, are much later, e. g. that described by O. Penzig in his *Contribuzione alla storia della Botanica*, Genoa, 1904. There is a similar copy in the University Library at Cambridge. Press Mark Ee. 5. 7 (Browne 1385).

plant paintings may still be traced in a fragment of a herbal of the eighth century discovered in the cover of an Armenian book [1] (Fig. 30), in the famous eighth-century Vatican MS. of Cosmas Indicopleustes,[2] in a very peculiar little manuscript of Nicander of the ninth century [3] (Plate IX), in the Paris and Cheltenham

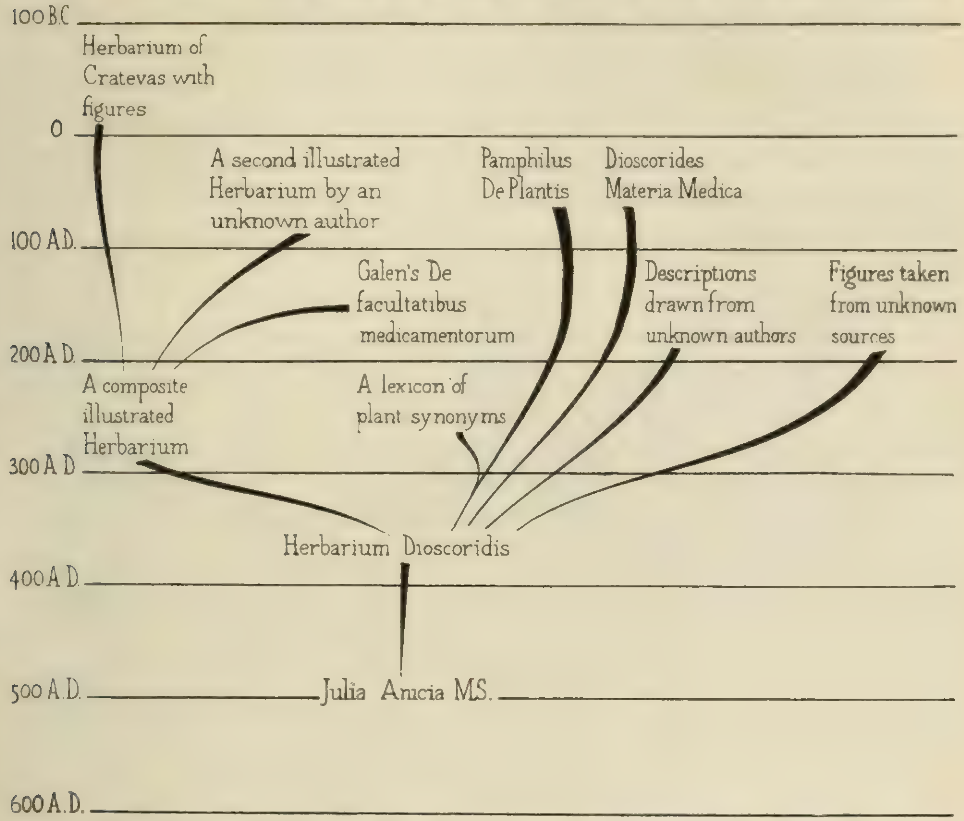



FIG. 29. THE GENEALOGY OF THE EARLIEST MANUSCRIPT OF DIOSCORIDES

Dioscorides MSS. of the ninth and tenth centuries respectively (Plates X and XVIII), in the Smyrna Physiologus of the eleventh century,[4] and in many other works of Byzantine origin. For the rest, however, the story of botany is mainly in the West and not in the East.[5]

[1] Discovered by F. C. Conybeare. Facsimile in MS. Bodley E. 19 (31528).

[2] Vat. Gr. 699. Reproduced in facsimile by C. Stornajolo, Rome, 1909.

[3] Bibl. nat., Supplément grec, 247.

[4] Josef Strzygowski, 'Der Bilderkreis des griechischen Physiologus', in the *Byzantinisches Archiv*, Heft 2, Leipzig, 1899.

[5] We do not here touch on the very interesting group of Dioscorides herbals of Persian origin, which are known from the twelfth century onwards. Chinese herbals are also known.

A few words may be devoted to the Greek text and manuscripts of Dioscorides. Modification and interpolation set in early and on an extensive scale. This process was encouraged by the alphabetical rearrangement of the paragraphs according to the name of the drug the properties of which are described. Such a rearrangement was made in the fourth, or perhaps in the third century. The oldest codices are of this type, and are thus of comparatively little *textual* value, though the illustrations that adorn them have the highest interest, alike for the history of Botany and of Art. On the other hand the non-alphabetical manuscripts are less ancient and, on the whole, less beautifully illustrated, but some of them are of high textual importance.

### *Alphabetical Greek Codices of Dioscorides*

The alphabetical Greek codices fall naturally into three classes:

I. This class contains the two most ancient manuscripts. Both are elaborately illustrated, text and figures being derived from one common archetype of the fourth century. The text is alphabetically arranged from beginning to end. It draws on the *Materia Medica* alone among the works of Dioscorides, but also contains certain works of other authors (as shown in Fig. 29). The figures illustrating the text of these manuscripts preserve the tradition of Crateuas. This class includes only:

(*a*) The *Julia Anicia*, written in capitals before 512, and known as the *Constantinopolitanus.* This manuscript was formerly at the Vienna Hofbibliothek, where it was numbered Med. Gr. I. It is now in St. Mark's Library at Venice (Plates VI and VII). The MS. is accessible in a beautiful photographic facsimile.

(*b*) The so-called *Neopolitanus,* written in half uncials in the seventh century. This manuscript was also formerly at the Vienna Hofbibliothek, where it was numbered Suppl. Gr. 28. It is also now in St. Mark's Library at Venice.[1]

II. This class contains a text borrowed not only from the *Materia Medica*, but also from the other works of Dioscorides. The text is divided, according to the subject, into five sections or books, within each of which the order is alphabetical. These sections treat respectively of (1) plants, (2) animals, (3) oils, (4) trees, (5) wines and stones. In (*c*) and (*d*) the text is com-

[1] There is an excellent but antiquated article on the *Constantinopolitanus* by L. Choulant 'Ueber die HSS. des Dioscorides mit Abbildungen', *Archiv für die zeichnenden Künste*, I, p. 56, Leipzig, 1855. In R. Dodoens' posthumous *Stirpium Historiae Pemptades sex,* Antwerp, 1616, figures on pp. 123, 126, 149, 288, 372, 377, 439, and 572 marked *e cod. Caesareo* are taken from the *Constantinopolitanus.*

bined with certain spurious works of Nicander. The figures, like those of class I, preserve the tradition of Crateuas with certain well marked differences. There is evidence that they spring from an archetype of not later than the fourth century.

(*c*) Cheltenham, Phillipps, 21975, tenth cent., now in the Pierpont Morgan Library, New York (Plate XVIII).

(*d*) Mount Athos, Laura Monastery, twelfth cent.

(*e*) Venice, St. Mark's Library, No. 92. Cotton paper, thirteenth cent.

(*f*) Escurial Σ.T. 17. Paper, fifteenth cent.

III. This class contains only the *Materia Medica* of Dioscorides

FIG. 30. 'Chamaepitys', from a fragment of an eighth-century Greek Herbarium [Bodl. E. 19]. The plant is probably *Ajuga reptans* Linn., the Creeping Bugle, or some allied species.

and has an alphabetical text not divided into books. No manuscript of this class is earlier than the fourteenth century.

(*g*) Milan, Ambros. A95 Sup., fourteenth cent.

(*h*) Rome, Vatican Urbinas 66, fifteenth cent.

(*j*) Venice, St. Mark's 272, fifteenth cent.

(*k*) Venice, St. Mark's 597, fifteenth cent.

and certain sixteenth-century manuscripts at Berlin, Paris, and the Escurial.

*Non-Alphabetical Greek Codices of Dioscorides*

The subject-matter of the remaining manuscripts of Dioscorides is not in alphabetical order. These manuscripts are very numerous and we need only mention the more interesting. They fall into two classes, of which the first is the more important for fixing the text as it contains only the genuine work of Dioscorides. In the second class this text is mingled with other material.

I. Manuscripts containing only works of Dioscorides. Among the most important are the following:

(*l*) Paris Bibl. nat. MS. gr. 2179, ninth century (Plate X).

This is the most valuable of all the manuscripts for fixing the text. It is beautifully illustrated and it is interesting to observe that the figures of the mandrake, that are anthropomorphic in all the other manuscripts, are not here given any human attributes. Thus these figures may contain elements in an even earlier tradition than the *Constantinopolitanus* and *Neopolitanus* and may take us back beyond the fourth century and perhaps beyond Crateuas. The manuscript is badly preserved and partially illegible, but is supplemented by

(*m*) Venice, St. Mark's 273, twelfth century, and

(*n*) Florence, Laurentian Plut. 74, 17, twelfth century.

(*m*) and (*n*) are parts of one manuscript copied from (*l*) or from a closely similar manuscript.

(*o*) Escurial III, R 3, imperfect, eleventh cent.

(*p*) Florence, Laurentian Plut. 74, 23. The only perfect manuscript of this class, fourteenth cent.

(*q*) Rome, Vatican Pal. 77. The older leaves of this manuscript represent the same tradition as (*o*), fourteenth cent.

II. The Greek manuscripts of Dioscorides that contain other works besides his are very numerous, but of little importance for the text. A few, however, have interesting figures. Among these are:

(*r*) Paris, Bibl. nat., MS. gr. 2180, fifteenth cent.

(*s*) Paris, Bibl. nat., MS. gr. 2182, written 1481.

The relation of the tradition of the figures to that of the text has not been investigated. It is a subject for a special research, and would be of great importance for the history of Art. From a first examination we may say that the line of descent of the figures may be traced as far as the thirteenth century along lines parallel to those of the text.[1]

To sum up the history of Greek botany, we may say that it probably arose with other sciences in the sixth century B.C., but that it does not come into clear view until the fourth century B.C. It was then firmly established as a scientific discipline, botanic gardens had been established, much research had been made, lectures were being given, and the work of one important writer has survived. By the first century B.C. scientific botanical

[1] The classification of the manuscripts given above is largely taken from M. Wellmann's masterly article on 'Dioskurides' in Pauly-Wissowa's *Real-Encyclopädie der klassischen Altertumswissenschaft*, vol. v, Stuttgart, 1905. Much information may also be obtained from H. Diels' 'Die Handschriften der antiken Ärzte. II. Teil. Die übrigen griechischen Ärzte ausser Hippokrates und Galenus', in the *Abhandlungen der königl. preuss. Akademie der Wissenschaften*, Berlin, 1906.

principles had been absorbed into agriculture and medicine, and their application is happily illustrated by works which survive from the two centuries which follow. Then begins a process of decay, but as late as the seventh century A. D. very careful and accurately illustrated herbaria were still being prepared. From then on only decadent imitations are known. Some of these, however, are remarkably accurate, and a study of them aids us in forming an idea of the botanical attainments of the earlier and the limits in the botanical knowledge of the later centuries.

### (*b*) *Botany in the West from the sixth to the twelfth century (The Dark Ages)*

During the Dark Ages, and even until the twelfth or thirteenth century, there were no effective additions to botanical knowledge. Up to that time we may say that such codified botanical knowledge as existed in the West was contained in a very small group of Latin works of which countless manuscripts existed. The versions of these works, their illustrations and their general appearance, present remarkable similarities in spite of their wide distribution. Three of them especially were often compounded in various proportions to form a single text. These three works are:

(i) The *Materia Medica* of Dioscorides.
(ii) The *Herbarium* of 'Apuleius'.
(iii) The pseudo-Dioscoridean *De Herbis Femininis.*

(i) The work of Dioscorides had already been translated into Latin in the time of Cassiodorus (490–585), who recommended the study of illustrated copies to such of his monks as were unable to read Greek.[1] The earliest surviving manuscripts of the Latin translation of Dioscorides are, however, of the ninth century.[2] Several of the manuscripts of the Latin Dioscorides contain remarkable illustrations.[3] The text has been printed in modern times.[4]

(ii) The *Herbarium* bearing falsely the name of Apuleius

[1] Cassiodorus, *Institutio divinarum litterarum*, c. 31 'Si vobis non fuerit graecarum litterarum nota facundia imprimis habetis herbarium Dioscoridis qui herbas agrorum mirabili proprietate disseruit atque depinxit.'

[2] The two oldest manuscripts of the Latin Dioscorides are at the Bibl. Nat., where they are numbered 9332 and 12995.

[3] Notably the Munich MS. Monac 377 of the tenth century in the characteristic Beneventan script. See H. Stadler, *Der lateinische Dioscorides der Münchener Hof- und Staatsbibliothek*, Janus iv, p. 548, Leyden, 1899.

[4] In Vollmöller's *Romanische Forschungen*, i, x, xi, and xii, Erlangen. Cf. also V. Rose in *Hermes*, viii, p. 38.

Platonicus was compiled in Latin perhaps during the fifth century. It describes very briefly the character and habitat, and at greater length the virtues of herbs, and it is possible that the name of Apuleius was attached to it because he was the author of a *Liber floridarum*, which, however, is of a totally different character. The *Herbarium* of Apuleius is in part taken from the real Dioscorides and in part from the work known as the *Medicina Plinii*. At an early date the text became associated with a lexicon of synonyms similar to that found in the alphabetical Greek manuscripts of Dioscorides. Some have seen in the character of these synonyms evidence of an African origin of this pseudo-Apuleian document. The earliest manuscript is of the sixth century,[1] and already exhibits these peculiar synonyms. There are a number of other manuscripts of the pseudo-Apuleius which contain a remarkably persistent group of illustrations becoming progressively more conventionalized.[2] The pseudo-Apuleian *Herbarium* has been printed a number of times, usually under the title *Apuleii Platonici de virtutibus herbarum*. The earliest edition was printed at Rome before 1484 by Philip de Lignamine. The printed editions of this work, like the manuscripts, vary greatly in their contents.

(iii) The *De herbis femininis* of the pseudo-Dioscorides was also written or rather compiled in Latin. It is only about half the length of the pseudo-Apuleius, and consists of a description of seventy-one herbs and their properties without the synonyms. It is drawn from Dioscorides, pseudo-Apuleius and Pliny, relying perhaps on a separate Latin version of the first of these. The earliest manuscript is of the ninth century,[3] but the work was probably put together no later than the sixth century, and perhaps in Italy during the domination of the Goths (493–555).[4] Manuscripts of this work in its pure form are not very common.[5] It has also been printed in modern times.[6]

[1] Leyden, Voss. lat. Q 9.

[2] Among the most ancient of these are the *Codex Hertensis* of the ninth century, described by Sudhoff in the *Arch. für Gesch. der Med.*, x, p. 265, Leipzig, 1916, and the *Codex Fuldensis* of Sudhoff of the tenth century at Cassel, where it bears the pressmark *Phys. et hist nat., fol.* 10.

[3] Rome, Barberini, ix. 29.

[4] M. Wellmann, ' Krateuas ', in *Abh. d. kgl. Gesellschaft d. Wissenschaften zu Göttingen, Philol.-hist. Klasse*, Berlin, 1897.

[5] Ten manuscripts of this work (one of which has been destroyed) are recorded by H. Diels, *Die Handschriften der antiken Ärzte*, Berlin, 1906.

[6] H. F. Kästner, ' Pseudo-Dioscoridis de Herbis feminis ', in *Hermes*, xxxi, p. 578, Berlin, 1896.

More interesting than these botanical works themselves are the paintings of plants with which a number of them are provided. These were copied from manuscript to manuscript, century after century, without direct reference to the objects intended to be represented, and thus arose definite traditions which can be traced in the representation of plants from age to age. The traditions of botanical illustration fall along somewhat different lines to those of the general history of manuscript illumination, and the figures of plants as they are encountered in manuscripts therefore merit a special study which has hardly yet been accorded them. In what follows we shall deal mainly with English developments which we have had most opportunity to study. There is evidence that the evolution of the herbal in other countries took a somewhat different course.

Our earliest illuminated Latin herbal is a sixth-century manuscript (Plates VIII, XX, and XXI) probably written in southern France. Most of its figures are already stylized and far removed from nature drawing. This model remained little altered until the tenth century, and herbals of that period from the Rhineland and from Italy still preserve the same traditional pictures.[1] By then, however, another treatment of plants becomes traceable in northern France. The figures in this new style have become symmetrical and heavy and some of them approach the manner which we describe below as Romanesque (Plate XXV).

An examination of a series of manuscript herbals of between the eleventh and thirteenth centuries reveals the fact that the figures fall in the main into two divisions, one of which we may call *Naturalistic* and the other *Romanesque.* Both these divisions, like other manifestations of Western culture, were doubtless derived from Italy, but it would appear that they reached the West by different routes, or at least with different cultural waves.

The naturalistic tradition of these Western herbals of the eleventh, twelfth, and thirteenth centuries probably took its rise in the south of Italy, perhaps in that region where developed the peculiar script to which the name of Beneventan has now become

[1] To this group belong Hertensis 192 written in Westphalia (?) in the ninth century (cf. Sudhoff, *Archiv für Gesch. d. Med.* x, p. 265, Leipzig, 1916), the Laurentian 73, 41 written in southern Italy in the ninth century, and Cassel Phys. Fol. 10 written at Fulda in tenth century. Munich Lat. 337, Italian work of the tenth century, belongs however to a quite different tradition.

attached. It drew on Greek models which were already beginning to undergo the change known as 'Byzantinism'. In line with this view of the southern origin of certain herbal illustrations is the fact that a medical work in the Anglo-Saxon language circulating in England in the twelfth century[1] was actually a translation of a known Salernitan document, and English leech-craft of the eleventh and twelfth centuries was profoundly influenced by Salerno.[2] It is of interest for our purpose to analyse the medical manuscripts in the Beneventan script that have actually survived. Some 600 Beneventan manuscripts (including fragments) are known, and nearly half of these are still lodged at the Benedictine monastery of Montecassino. The manuscripts vary in date from the early ninth to the mid-thirteenth century and thus represent the period during which Salerno was an important medical school. Of the Beneventan MSS., 14 (13) or about 2 per cent. are medical.[3] Of the 14 surviving early southern medical manuscripts we have adequate details of 10, and all of these are of works which were in fact influential in establishing the main currents of Dark Age medicine and nearly all of them contain Herbaria. The most important perhaps was the recently destroyed Turin codex of the eleventh century. The figures in this manuscript, a few of which have been fortunately preserved in photographs, were of a definitely Byzantine cast and must have been derived from an earlier Greek original, and the influence of this manuscript, or one very closely resembling it, can be traced in some English herbals.[4] (Fig. 31.)

The first herbals brought to this country had however probably been prepared in northern France (cf. Betony in central figure of Plate XXV with the same plant of Plate XVII). But the models from which the artist worked must have betrayed the fullest evidence of an origin from a region further south, for traces of a Mediterranean flora may still be clearly discerned in the English copies (cf. Henbane in Plate XVI). During the tenth, eleventh, and twelfth centuries there developed a very characteristic English school of draftsmanship and this new manner was not without its influence

[1] Max Löweneck, 'Περὶ διδάξεων, eine Sammlung von Rezepten in englischer Sprache', in the *Erlanger Beiträge zur englischen Philologie*, ix, Erlangen, 1896.

[2] Charles Singer, 'A Review of the Medical Literature of the Dark Ages with a new text of about 1110', *Proceedings of the Royal Society of Medicine* (*section of the History of Medicine*), x. 102, London, 1917.

[3] The known Beneventan MSS. are recorded by E. A. Lowe in *The Beneventan Script, a study of the South Italian minuscule*, Oxford, 1914.

[4] Especially in the MS. Harley 5294.

PLATE XVII. Paris, Bib. nat. MS. lat. 6862 Apuleius Xth cent. French work


fo. 20 v *Vetonica* = Betony

fo. 26 v *Verminacia (columbaris)*
= *Verbena officinalis*

on the paintings that adorned the herbals (cf. figures of Orobus and Teucrium on Plate v and of Ivy on Plate XXIII).

But this native style was, in turn, largely replaced by the Romanesque. The change in the figures was accompanied by a well-marked alteration in the character of the script. These changes took place in England a generation or two after the


FIG. 31. OUTLINES OF THE MANDRAKE AND ITS GATHERERS

Traced from a twelfth-century English Herbarium in the British Museum and a contemporary Italian Herbarium to illustrate the close similarity.

Conquest, which introduced a revolution into art and letters at least as great as that to which it subjected the social system of this country.

On the Continent the Romanesque method of plant illustration had established itself at an earlier date. This style originated or at least emanated from northern and north-eastern France. Its ultimate origin appears to be a debased style of Roman art proceeding from northern Italy and perhaps modified in Carolingian times by English influence. Its characteristics, so far as plants are concerned, are a replacement of free drawing by symmetrical

design (Plate v, central figure), while an increased formalism is ultimately obtained by the enclosure of the picture in a rectilinear frame (Plate xxv, lateral figures). As the style develops, a background at first simple and afterwards more elaborate is provided. The human body is often drawn half naked or even completely nude, and the drapery arranged along characteristic lines (Plate xxiv).

Comparing the styles we may characterize the Italian as more free and the Romanesque as more formal. Both styles are trace-


FIG. 32. THE PLANTAIN
From a thirteenth-century Herbarium in Romanesque style prepared in England. Sloane, 1975, fo. 12 v.

FIG. 33. PLANTAIN
From the *Herbarius latinus* printed at Mainz in 1484.

able even in the later manuscripts and are carried over into the early printed herbals. Some manuscripts even contain specimens of both styles, and in all herbals the centaur holding a plant is a favourite device. It is noteworthy that in none of the English herbals is there any trace of the influence of the characteristic Celtic school of illumination, while the peculiar and well-known Anglo-Saxon style of draftmanship is far less obtrusive in the books of plant pictures than in ecclesiastical documents. The influences that produced the herbals were, in fact, mainly of a lay character, but nevertheless a certain characteristic English manner of plant representation can be detected in them. This English manner had completely disappeared by the beginning of the thirteenth century (cf. Sloane 1975, Plates xv and xxv).

(*c*) *Botany in the West from the twelfth to the fifteenth century (The Middle Ages)*

After the long depression of the Dark Ages the study of plants began to take an upward turn at a much earlier period than the sister study. The revival begins with literary rather than scientific effort, for neither the poem called that of Macer Floridus composed by Odo of Meune (died 1161), nor the account of plants in the later

FIG. 34. WALL-FLOWER WITH DODDER
Cf. Orobus on Plate v.

FIG. 35. YELLOW FLAG

From the German version of the *Hortus Sanitatis*, printed at Mainz in 1485.

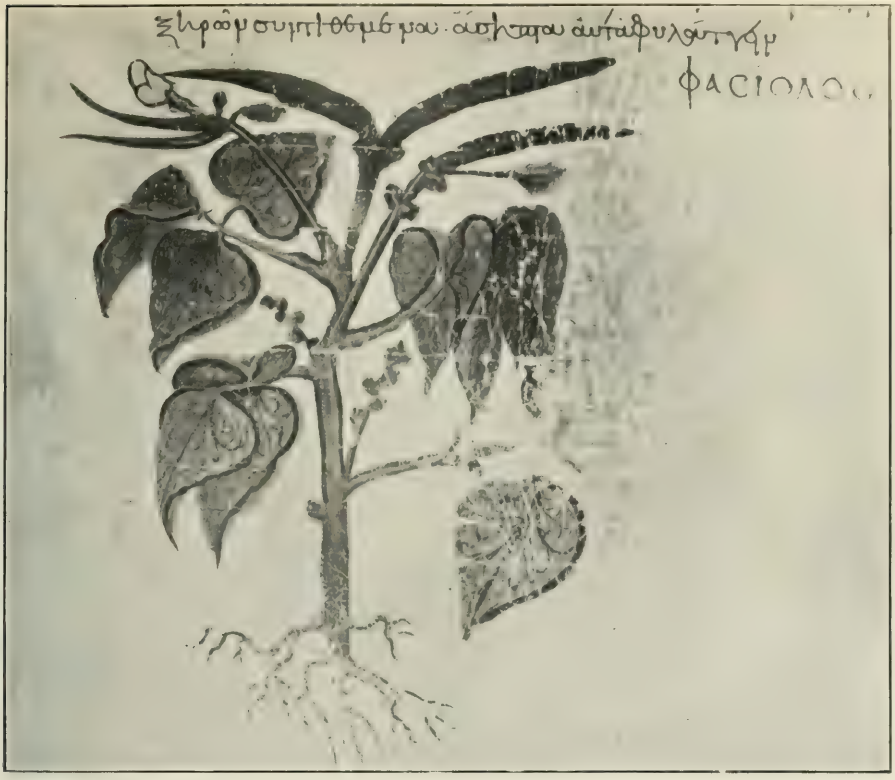
*Subtilitatum diversarumque creaturarum liber* (early thirteenth century), wrongly attributed to Hildegard of Bingen, contains evidence of new or even direct observation. More scientific in spirit is the somewhat later work *De vegetabilibus* of Albertus Magnus (1206?–80). This is primarily a compilation based on the peripatetic work on plants by Nicholas of Damascus (first century B.C.), of which only fragments remain. Albert's treatise is essentially a learned product and its author is hampered by his desire to fit the nature of plants into an ill-thought-out system, while his work is marred

by the scholastic doctrine that '*philosophia* is concerned with generalities, not particulars'. It is a phrase which itself explains the failure of scholasticism to erect an enduring scientific structure. But in spite of such a handicap the work of this extraordinary man contains evidence of a certain amount of careful first hand observation. His botanical treatise gives a not unpleasing impression of the common sense and open-eyedness of the great mediaeval scholar who has a place among the grandfathers, if not among the fathers, of modern botany. We translate one of his chapters as a good specimen of mediaeval descriptive botany.

'Of the Oak and its qualities.

'The oak is a very large and tall tree with broad branches; it has many roots which go deep down and when old has a very rough bark, but the young tree is smooth. It has great breadth and size in its branches; when it is thriving its leaves are thickly set, broad, and hard. The leaves are wholly surrounded by triangles, the bases of which are upon the leaf and the angle at the exterior. Many leaves are attached to it, but they fall off. When they are dried up, however, they still sometimes cling in large numbers. Its wood increasing by straight layers is composed of straight pores, can be split to a line and can be hewn and retains well the shapes of large incisions, but in this box-wood surpasses it. The outer zone is of a pale colour but towards the centre it shades into a reddish tint. If it is put in water, at first it swims, but at last sinks owing to its earthy nature and then grows black. Its fruit is called acorn (*glans*); it is not joined by a stalk of its own to the branch on which it grows, but small cups spring out from the branches and in these the acorn is formed. The acorn also has outside a hard shell in which it is enclosed; this resembles well-polished wood, shaped like a column except that its apex is not a plane superficies but a hemisphere and has a point in the middle to represent the pole. Below is the base of the acorn through which it draws nourishment from the little cup; that also is not simply a plane superficies but is somewhat flattened at the pole; this depression is formed by the weight of the acorn, for if it were an exact hemisphere there would be no place for the reception of nourishment except the point, and through that it could not receive enough. The acorn within the sheath is surrounded by rind, not hard but soft, which is formed from the excretion of the acorn; the acorn is twisted round itself and divided down the middle as a column might be cut lengthways by a plane surface. At the apex, however, is the life germ and what is beneath is of a floury substance and is to be regarded as matter and food for the germ. The little cup in which the acorn itself sits is concave and evenly formed as if it were smoothed on a lathe inside. The bottom is somewhat levelled since from it the acorn draws its nourishment; on the outside it is rough

PLATE XVIII. From the Phillipps Dioscorides MS. Xth c.



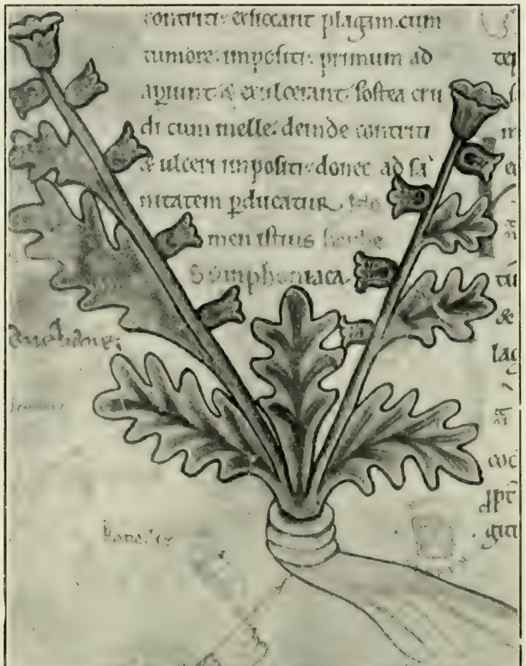
fo. 186 v ΦΑϹΙΟΛΟϹ SEEDLING BEAN. Cf. PLATE VII

fo. 34 v ΓΟΓΓΥΛΗ TURNIP

Now in the Pierpont Morgan Library New York

HYOSCYAMUS

From MS. Harley 1585 fo. 19 v



English, about 1200

PLANTAIN

From British Museum MS. Add. 17063 fo. 4 r

Italian, of about 1450
Compare Text Fig. 36

DRACUNCULUS

From MS. Harley 1585 fo. 22 v

English, about 1200

PLATE XIX

because of its own earthy nature which is expelled from the material of the acorn. It is not joined by any sort of stem to the branch but sits immediately upon it. This is to prevent the acorn from being too far distant from the branch, because if it had to draw its food a long way, it would become hard and cold and would do no good, especially as the juice of this tree is very earthy.

On the leaves of the oak often grow certain round ball-like objects called galls, which after remaining some time on the tree produce within themselves a small worm bred by the corruption of the leaf. If the worm exactly reaches the midst of the gall apple weather prophets foretell that the coming winter will be harder: but if it is near the edge of the gall they foretell that the winter will be mild. . . . Galls have a juice pure in itself as long as the apple is green and moist; but when it is rubbed against a flat clean piece of iron it immediately is transformed into a kind of very black encaustic.

'The leaves of the acorn are extraordinarily astringent but less dry. The acorn resembles the chestnut in that both are astersive and cause flatulence in the lower bowel; both strengthen the limbs and both are good food especially for pigs. Galen says that the acorn as well as the chestnut is good for nourishment and deserves more praise than all the fruits of growing trees: but the chestnut is more nutritious than the acorn on account of its greater sweetness. But the food they afford lacks the praise of men because it is too astringent, but if chestnuts are mixed with sugar they make good food; taken otherwise they will be of slow digestion but the oak is even slower. The astringency in the inner bark of the acorn is greater than in the acorn itself. The leaves of the oak ground to powder and laid upon wounds make the flesh unite. The acorn is of as much value as the chestnut as a remedy against poison.

FIG. 36. THE PLANTAIN
From Lignamine's Apuleius, Rome, 1483, printed probably from metal blocks.

'The juice of galls darkens the hair: powder of them gets rid of superfluous flesh and warts. Galls are helpful also if placed on decayed spots of the teeth and in many other medical operations which must be determined in (books on) simple medicines.'[1]

[1] Translated from Albert's *De vegetabilibus*, Lib. VI, Tractatus 1, *De arboribus*, heading *Quercus*. There is a good appreciation of Albert as a botanist in E. H. F. Meyer's *Geschichte der Botanik*, vol. iv, p. 28. Königsberg, 1857.

In this chapter Albert alludes to the relationship of galls to insects. This was not mentioned by Aristotle and hardly suspected by the ancients, being but faintly referred to by Pliny. The subject had to await the microscopic researches of Malpighi to gain further elucidation. In the same passage Albert distinguishes the two cotyledons of the acorn, gropes after a botanical nomenclature adequate to describe plant forms, and succeeds in giving a description which would convey some definite picture to one who had never seen the plant. We can at least say that in the hands of Albert botany has begun to move on the upward grade toward the level at which Theophrastus left it.

Yet the scholastic movement, to the furthering of which Albert's chief efforts were directed, was of its nature inimical to the first-hand study of plants and animals. The thunders of the contest between Nominalism and Realism might well drown the still small voice with which Nature calls for direct observation. The great scholastic centuries from the twelfth to the fifteenth are hardly more fertile than the previous period in botanical writing exhibiting any first-hand knowledge. In one department of intellectual activity, however, there was some clear revival of the spirit of the naturalist. The artistic spirit early showed its kinship with the scientific by the closeness with which some illuminators of manuscripts sought to imitate nature. The herbarium itself remained a fixed text unaltered from that inherited from the preceding age, but its illustration underwent a definite development in the direction of increased naturalism. A close study of some of these beautiful works shows that the early printed herbals had predecessors, and that already in the thirteenth century the older merely stylistic method of plants was giving place to a real attempt to represent nature.

To explain the development of illustration in the herbarium in the later mediaeval centuries some reference must be made to the mode in which these volumes were prepared. The text was usually written before the figures were inserted, and writing and illumination were the work of different hands. Thus the figures are as a rule a little later and may occasionally be much later than the text. Sometimes the provenance of the model can be determined from an examination of the figures. Thus in one of the figures from the Anglo-Saxon herbarium of about the year 1000, the plant representing the henbane, or to call it by its earlier English name, *hennebelle*, is not our familiar *Hyoscyamus niger* but *Hyoscyamus*

*reticulatus*, a Mediterranean form not found in this country (Plate XVI). The illustrator must have had an herbarium of a far southern tradition before him from which he copied his figures. Another proof of the way these plant illustrations were copied has been afforded by Fig. 31, which shows the mythical mandrake with its roots shaped in human form. The form on the left is traced from a manuscript prepared in southern Italy, that on the right from a manuscript made in England in the twelfth. The English figure is clearly based on an Italian original.

Now it must be remembered that those who used these herbaria had no idea of plant distribution. The conception that flora had local peculiarities had been familiar enough to Aristotle and Theophrastus, and traces of it can be found in Pliny and Dioscorides, but the idea had been almost lost in the Middle Ages and remained obscured until revived by Euricius Cordus. When the scribe copied his text he was accustomed to leave a space of a particular size and shape into which the illustrator could then fit his figure, and many herbals have come down to us in which the illustrator has either not completed or not begun his work, so that these spaces remain blank. The gaps might be filled in later, sometimes centuries later, according as the owner of the book had the talent or the financial resources at his disposal. Sometimes the original model was not available when the later figures were inserted, so that these do not fit the spaces left for them.

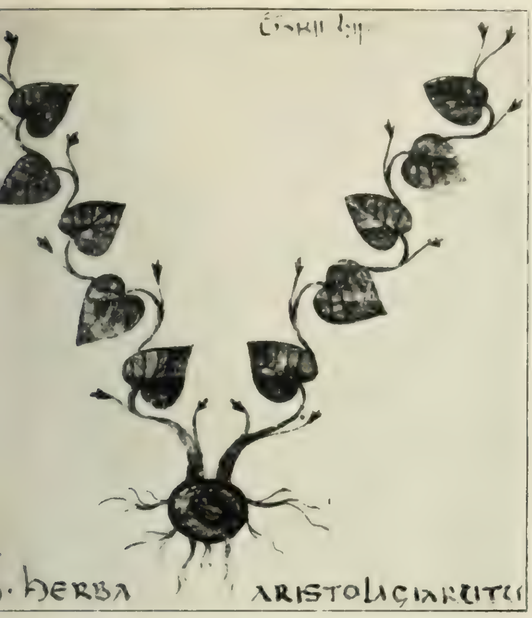
Such figures of plants were usually copied from earlier figures and therefore became further removed from nature at each stage. But the degradation of the copied herbarium had its limits, and those limits were reached when the figures had so utterly deteriorated that no semblance to an indigenous plant could be discerned by the native scribe or owner of the book. At this point it was necessary to return to nature and to give some impression of a real and local plant, though not necessarily that originally intended by the author of the text. The point of lowest illustrational degradation appears to have been generally passed with the full development of the Romanesque manner at the end of the twelfth or the beginning of the thirteenth century, and from this period we can trace the rise towards modern botany. But individual instances can be adduced of the onset of the change at an even earlier date and there are figures of plants of the first half of the twelfth century which show a definite upward tendency (Bodley

130, Plates v, xv, and xxiii). The movement was continuous and in manuscripts of the fourteenth and fifteenth centuries we can distinguish beautiful attempts to imitate nature comparable in their degree to the work of the artists who employed their talents on grander schemes (Plate xi, especially margins). Such painters of plants as Leonardo da Vinci and Albrecht Dürer had, therefore, their humbler craftsmen predecessors.

These general statements require some modification, for not only did the naturalistic school of plant illustration produce good models in the twelfth century, as we have already shown, but also in the thirteenth, fourteenth, and fifteenth centuries, side by side with real artistic efforts, are still to be found the crudest and most wooden imitations of the outworn models. So it is also with the earlier printed herbals. Some of these are mere repetitions of ancient diagrams, some are real attempts to represent plant life as it is, some are a mixture of the two types of illustration, and some contain illustrations which are themselves a mixture of the two types. The early printed herbals therefore present a stage of development that can be paralleled in the manuscripts, and it is thus perhaps unfortunate that historians of botany have usually elected to begin their accounts with these printed works. Botany is perhaps alone among the sciences in that it is possible to tell its history as an almost continuous tale, and the invention of printing introduces no specially new element into that tale nor does it mark an important period in it.

Among the purely traditional pictures in the incunabula we may class that of the plantain from the Latin Herbarium printed at Rome in 1484, which we can exactly parallel from an Italian manuscript of somewhat earlier date (Fig. 32, p. 72, and Plate xix). As a naturalistic representation we may place that of the wall-flower surrounded by dodder from the German version of the *Hortus sanitatis* of Mainz, 1485 (Fig. 34, p. 73), beside a figure of an Orobus from a herbarium written and illustrated in England early in the twelfth century (Plate v). As an intermediate form we may regard the plantain from the Latin *Herbarius latinus* of Mainz of 1484 (Fig. 33, p. 72) and compare it with the Anglo-Saxon effort of about 1000 on the one hand (Plate xvi) and the stereotyped thirteenth-century forms on the other (Fig. 32, p. 72). An instructive series is also provided by the plants henbane (Plates viii, xv, xvi and xix), Betonica (Plates v, xvii, and xxv), or Dracunculus (Plates xix, xxi, and xxii).

PLATE XX. From Leyden MS. Voss Lat. Q.9 Apuleius VIth cent.




fo. 66 v *Aristolochia*


o. 92 v *Heliotropia* = Forget-me-not


fo. 72 v *Camillea* = Teasel

Plate XXI. Leyden MS. Voss Lat. Q 9 Apuleius VIth cent.


fo. 60 v *Dracontea* = *Dracunculus vulgaris*

A Mediterranean Species

From the beginning of the sixteenth century the development of the Herbarium may be followed in several readily accessible works, and we need therefore pursue it no further.[1]

## VI. The Botanical Results of Theophrastus compared with those of Early Modern Botanists

### (a) *Nomenclature and Classification of Plants*

We may now return to glance at the botanical work of the Lyceum. If we would realize the course of ancient botany we must mentally sever two ideas which we have inherited in combination, viz. the description of plants and the system of classification of plants. Unlike investigators of animal forms, botanists of the third and fourth centuries B.C. had not developed a system of classification. It is true that some would discern the idea of plant families in the descriptions of Theophrastus, but it is hardly possible to draw from his works a botanical table such as the zoological table that we have extracted from Aristotle. Still less shall we find in Theophrastus a definite technical nomenclature. Even Dioscorides who lived four hundred years later and whose names for plants still form the larger part of the popular English botanical vocabulary is scarcely more advanced in this respect.

In the absence of any adequate nomenclature or classification the work of Theophrastus gives at first a confused impression. It is a descriptive treatise seldom illumined by the philosophic flashes characteristic of the Aristotelian biological writings, and its author, unequipped with the exact terms with which we can now describe plants and parts of plants, seems to be working under insuperable disadvantages. Yet if we take trouble to comprehend his method we shall see that he has produced a very scientific and thorough piece of investigation. Not only are the descriptions almost always accurate and the illustrations apposite, but the writer is unusually careful to distinguish his own observations from those which he has merely heard, and to separate hypotheses from the records of observations. In this respect Theophrastus is unequalled among Greek biologists. But his work, like all Greek biology, is marred by the almost complete absence of any account of the processes of investigation. Compared to a modern

[1] Agnes Arber, *Herbals* (Cambridge, 1912), and J. F. Payne, 'The Herbarius and Hortus Sanitatis' (*Transactions of the Bibliographical Society*, vi. 63. London, 1903). A. C. Klebs, 'Herbals', in *Papers of the Bibliographical Society of America*, xi, p. 75, Chicago, 1917.

scientific work it is therefore but a fragment containing the conclusions only.

We have said that Theophrastus has no system of classification. It would perhaps be better to say that he suggests many systems but that he has discovered no natural system, and is fully aware of this. He develops, however, several tentative methods of dividing the kinds of plants. The one which he found in practice most effective was a division into trees, shrubs, under-shrubs, and herbs. Other distinctions are the common popular divisions into wild and cultivated, flowering and flowerless, fruit-bearing and fruitless, or again aquatic, terrestrial, marsh-living, and marine. In all this there is no effective and permanent scheme of arranging plant forms, and this defect he shares with all the older botanical writers.

Yet the methods of arranging plants adopted by Theophrastus, imperfect as they are, were hardly improved upon for nearly two millennia. In the illustrated manuscript herbals there is seldom any grouping of plants according to their structure. They are usually either in alphabetical order or placed according to their uses in healing or in some other artificial manner. In Dioscorides, however, plants are roughly grouped in some places according to their form, and occasionally he presents us with a series belonging to the same family, e.g. the *Compositae*, the *Labiatae*, or the *Leguminosae*. The same tendency is also encountered in the fine Anglo-Saxon Herbarium of about A. D. 1000 extracted from the Herbaria of Dioscorides and Apuleius. In this work there is a real grouping of Umbelliferous plants, and in other respects there are traces of a rudimentary attempt at a system of classification. It is a point that does not perhaps appear so clearly from the printed text as in the manuscript itself, but there is every reason to suppose that such arrangement as exists cannot be placed to the credit of the Anglo-Saxon leech, but to the compiler of the work from which he was translating.

In general it may be said that we encounter no real classification until towards the end of the sixteenth century. Among the sixteenth-century writers the tendency to group plants according to their physical characteristics advanced only with extreme slowness. The herbal of Brunfels (1484–1534) that appeared in 1530 [1] is no more systematically arranged than Dioscorides, while that of

[1] Otto Brunfels, *Herbarum vivae icones*, 3 parts, Strasburg, 1530–40; also the *Contrafayt Kreüterbůch*, Strasburg, 1532

Fuchs (1501–66) dated 1542[1] is merely alphabetical. Indeed the rudimentary classificatory method exhibited by Theophrastus is hardly attained even by the work of Bock ( = Tragus, 1498–1554) of 1546.[2] The herbal of Bock is divided into three parts, the first and second containing the smaller herbs, the third the shrubs and trees. In Bock's work the feeling for relationship is confined to smaller groups as with Theophrastus, from whom indeed he inherits them.

No satisfactory basis of classification was in fact forthcoming until the appearance of a description of a few plants in the *Historiae Stirpium* by Valerius Cordus (1515–44), published posthumously in 1561.[3] Cordus was the first to suggest the structure of the flower as a basis of classification, but his work was largely disregarded until reprinted in the eighteenth century, embedded in the botanical writings of Conrad Gesner (1516–65),[4] who had adopted and developed the views of Cordus. As regards precedence of publication, the first modern botanist to attain even to the low classificatory level of Theophrastus was probably Charles de l'Ecluse (Clusius, 1526–1609), the books of whose *Rariorum plantarum historia* of 1576 [5] are divided to some extent according to the plants of which they treat, the first of trees, shrubs, and undershrubs, the next of bulbous plants, the third of scented flowers, the fourth of scentless flowers, the fifth of poisonous, narcotic, and acid plants, and the sixth of a group containing plants with milky juice, *Umbelliferae*, ferns, grasses, *Leguminosae*, and some Cryptogams.

In de l'Obel (1538–1616) [6] and Cesalpino (1519–1603) [7] we encounter at last two writers who have a preponderating interest in the arrangement of plants according to their natural affinities. The primary divisions of de l'Obel are the traditional ones, trees, herbs, &c., and Monocotyledons and Dicotyledons are dis-

[1] Leonhard Fuchs, *De historia stirpium commentarii insignes*, Basel, 1542.

[2] Hieronymus Bock, *New Kreutter Buch*, Strasburg, 1539 ; 2nd edition, with figures, Strasburg, 1546.

[3] Valerius Cordus, *In hoc volumine continentur Valerii Cordi Annotationes in Pedacii Dioscorides . . . eiusdem Val. Cordi historiae stirpium. . . . Omnia Conr. Gesneri collecta*, Strasburg, 1561.

[4] Conrad Gesner, *Opera botanica*, 1751.

[5] Charles de l'Ecluse, *Rariorum aliquot stirpium per Hispanias observatarum Historia*, Antwerp, 1576.

[6] Mathias de l'Obel, *Plantarum seu stirpium historia*, Antwerp, 1576.

[7] Andrea Cesalpino, *De plantis*, Florence, 1583.

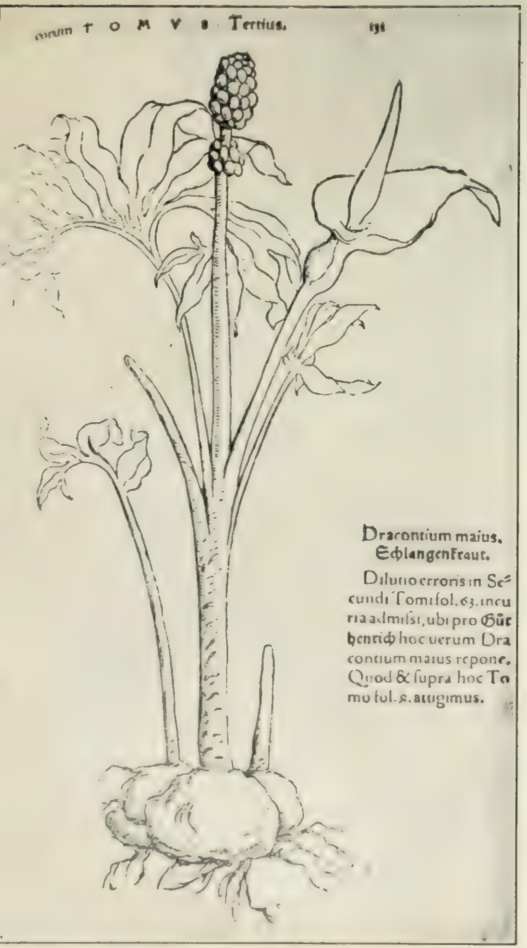
tinguished about as clearly as by Theophrastus. His system of classification, like much of Theophrastus, is largely based on leaf form, but he shows real advance in the synoptical tables that he constructed for the diagnosis of plant forms. Many of these tables betray a knowledge of true natural relationships. Cesalpino brings us into another realm; he pays much attention to the fruit, seed, and flower for purposes of classification, and distinguishes clearly though with insufficient emphasis between monocotyledons and dicotyledons. He has at last passed definitely beyond Theophrastus.

In the absence of any adequate system of classification almost all botany until the seventeenth century consisted primarily and mainly of descriptions of species. To describe accurately a leaf or a root in the language in ordinary use would often take pages and overwhelm the reader by its bulk. Modern botanists have invented an elaborate terminology which, however hideous to eye and ear, has the crowning merit of helping to abbreviate scientific literature. Botanical writers previous to the seventeenth century were substantially without this special mode of expression. It is partly to this lack that we owe the persistent attempts throughout the centuries to represent plants pictorially in herbals, manuscript and printed, and thus the possibility of an adequate history of plant illustration.

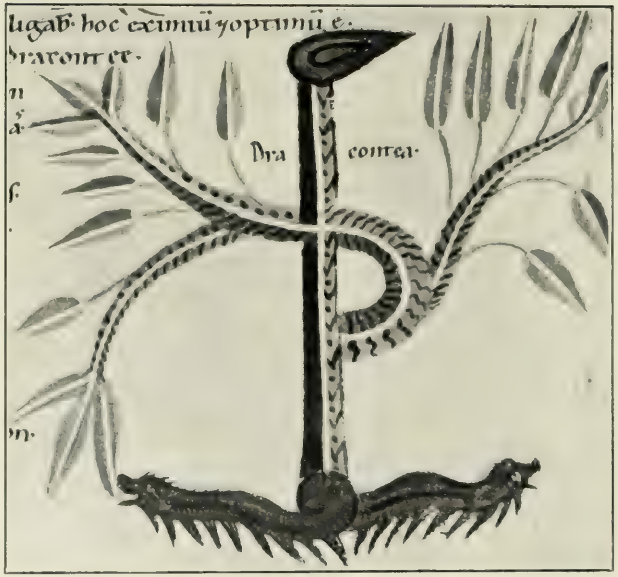
Theophrastus seems to have felt acutely the need of botanical terms, and there are cases in which he seeks to give a special technical meaning to words in more or less current use. Among such words are *carpos* = fruit, *pericarpion* = seed vessel = pericarp, and *metra*, the word used by him for the central core of any stem whether formed of wood, pith, or any other substance. Thus he speaks of 'the seed belonging to the *carpos* (fruit): by *carpos* is meant the seeds bound together with the *pericarpion* (seed-vessel)'.[1] It is from the usage of Theophrastus that the exact scientific definition of fruit and pericarp has come down to us.[2] We may easily discern also the purpose for which he introduces the term *metra*, a word meaning primarily the *womb*, into botany and the vacancy in the Greek language which it

[1] *Historia plantarum*, i. 2, i.

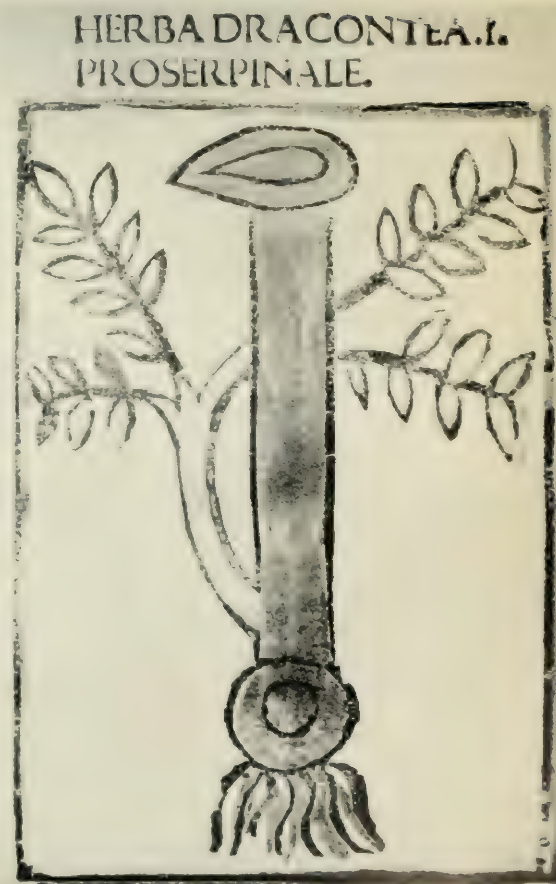
[2] Though it is possible that Theophrastus derived it from Aristotle. Cp. *De Anima*, ii. 1, 412$^b$ 2. In the passage τὸ φύλλον περικαρπίου σκέπασμα, τὸ δὲ περικάρπιον καρποῦ in the *De Anima* the word does not, however, seem to have the full technical force that Theophrastus gives to it.




O. Brunfels, *Herbarum Vivae Icones*, Vol. III. p. 131
Strasburg, 1536




MS. Harley 4986 fo. 7 v
German work of end of XIIth cent.

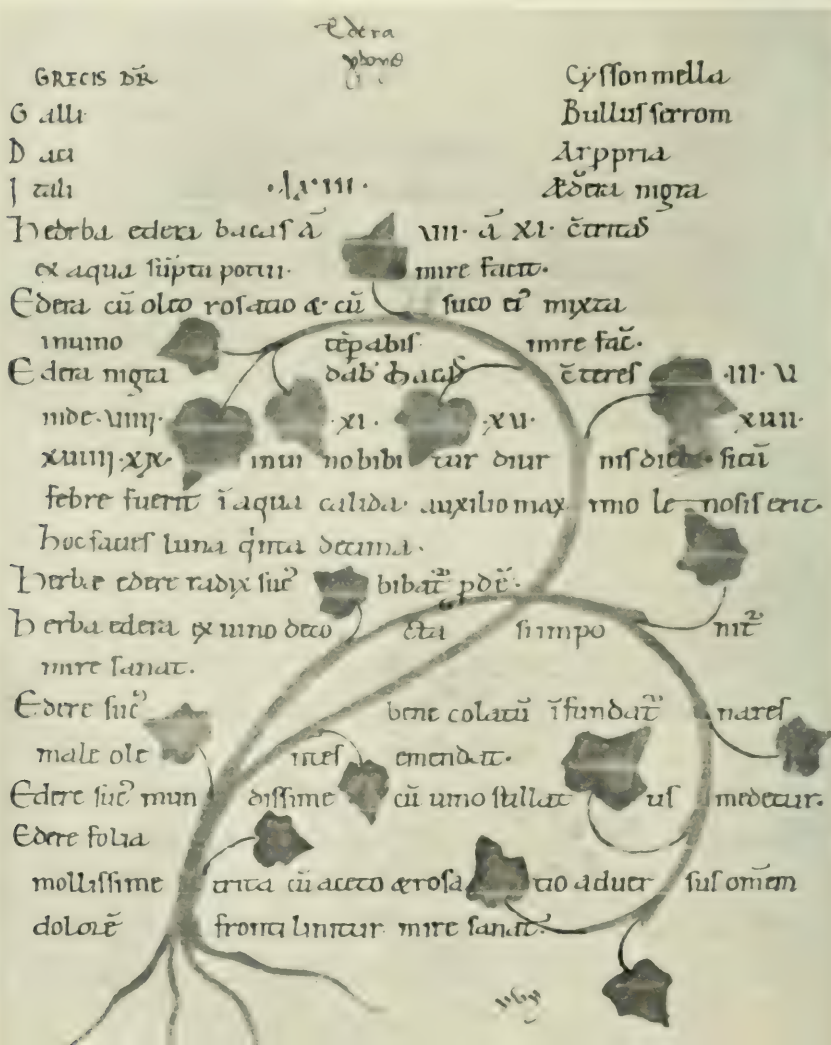



Apuleius, printed Rome, 1483, from metal (?) blocks by
De Lignamine

PLATE XXIII

MS. Bodley 130 written in St. Albans 1120


fo. 55 r *CRISOCANTUS* = IFIG = IVY (Flowering form)



was made to fill. '*Metra*', he says, 'is that which forms the middle of the wood, being third in order from the bark and corresponding to the marrow in bones. Some call this part the *cardian* (heart), others call it the *enterionen* (inside), others again call only the inner part of the metra itself the *cardian*, while others distinguish this as marrow.'[1] He is clearly inventing a word to cover all the different kinds of core and importing it from another study. This is the method of modern scientific nomenclature which hardly existed for the sixteenth-century botanists. The real foundations of our modern nomenclature were laid in the later sixteenth and in the seventeenth century by Cesalpino and Joachim Jung.

### (*b*) *Generation and Development of Plants*

Theophrastus understood the value of developmental study, a conception that he must have derived from his master Aristotle. 'A plant', he says, 'has power of germination in all its parts for it has life in all its parts, wherefore we should regard them not for what they are but for what they are becoming.'[2] The various modes of plant reproduction are correctly distinguished as they would be by any farmer. 'The ways in which trees and plants in general originate are these: spontaneous growth, growth from a seed, from a root, from a piece torn off, from a branch or twig, from the trunk itself; or again from small pieces into which the wood is cut up.'[3] The spontaneous origin of living things was taken for granted by Aristotle and was hardly questioned until the sixteenth century, when, in a flash of genius, Fracastor (1478 ?–1553) suggested that the supposed spontaneous generation was really a process arising from undiscovered seeds;[4] the suggestion did not gain demonstration until the experiments of Redi (1626–94) in the seventeenth century.[5] It is therefore the more interesting to find an inkling of this idea in the mind of Theophrastus. 'Of these methods', by which plants originate, he says, 'spontaneous growth comes first, one may say, but growth from seed or root would seem most natural; indeed

[1] *Historia plantarum*, i. 2, vi.

[2] *Historia plantarum*, i. 1, iv. [3] *Historia plantarum*, ii. 1, i.

[4] Girolamo Fracastoro, *De contagionibus et contagiosis morbis*, Venice, 1546. See also Charles and Dorothea Singer, 'The Scientific Position of Girolamo Fracastoro,' *Annals of Medical History*, i, New York, 1917.

[5] Francesco Redi, *Experimenta circa generationem insectorum*. Amsterdam, 1671.

*these methods too may be called spontaneous*; wherefore they are found even in wild kinds, while the remaining methods depend on human skill or at least on human choice.' [1]

There are other passages in which Theophrastus expresses some doubts as to the existence of spontaneous generation.[2] He quotes the view of Anaxagoras (*c.* 450 B. C.) who thought that the air contained seeds (σπέρματα) of all the things, plants among them, that make up the visible universe, and contrasts this theory with that of other philosophers who held that plants and animals were generated *de novo* from special combinations of the elements, and then he observes that 'this kind of generation is somehow beyond the ken of our senses. There are other admitted and observable kinds, as when a river in flood gets over its banks . . . and in so doing causes a growth of forest in that region that by the third year casts a thick shade', an event which he assures us took place at Abdera. The same results may be brought about by heavy rains.

'Now, as the flooding of a river, it would appear, conveys seeds of fruits of trees . . . so heavy rain acts in the same way; for it brings down many of the seeds with it, and at the same time causes a sort of decomposition of the earth and the water. In fact the mere mixture of earth with water in Egypt seems to produce a kind of vegetation. And in some places, if the ground is merely lightly worked and stirred, the plants native to the district immediately spring up.' [3]

The process of germination of seeds is one that must have awakened admiration from a very early date. Even the Egyptians, a people by no means observant of the minute phenomena of plant life, were struck by it, and in a bas-relief, put up to record one of the Syrian expeditions of Tethmosis III (about 1500 B.C.), we may discern a series of figures illustrating the development of seedlings (Fig. 37). It is thus by no means remarkable that the process should have impressed Theophrastus, who has left on record his views on the formation of the plant from the seed.

'In germinating some of these plants produce their root and their leaves from the same point, some separately from either end of the seed. Wheat, barley, spelt, and in general all the cereals produce them from either end, in a manner corresponding to the

[1] *Historia plantarum*, ii. 1, i.

[2] Theophrastus seems, nevertheless, to accept fully the doctrine of spontaneous generation in the *De causis plantarum*, i. 2.

[3] *Historia plantarum*, iii. 1, v.


nes. perfectissime sanat. Nomina istius
herbe. Centauria maior.
huius genera sunt duo.
has autem centaurias
duas chironcentauros di
citur invenisse. inde et
nomine ipsius centau
ri nomen est ipositum.
Ipse vero de his herbis
herbis medicinam instru
it primusque egrotan
tibus tradidit.
Quidam vocant eam



perfectissime sanat.
Homerus.
Mercurius.
dicitur.
AR
chia
ter
Nomina istius herbe.
Electropion. Duas
maximas virtutes habet.
A grecis dicitur
Alii: Dia
Alii: Scorpion. Alii: Eraclea

position of the seed in the ear, the root growing from the stout lower part, the shoot from the upper part; but the part corresponding to the root and that corresponding to the stem form a single continuous whole. Beans and other leguminous plants do not grow in the same manner, but they produce the root and the stem from the same point, namely, the point at which the seed is attached to the pod, which, it is plain, is a sort of starting-point of fresh growth. In some cases there is a process, as in beans, chick peas, and especially lupines, from which the root grows downwards, the leaf and stem upwards.

'There are then these different ways of germinating; but a point in which all these plants agree is that they all send out their roots at the place where the seed is attached to the pod or ear, whereas the contrary is the case with the seeds of certain trees, as almond, hazel, acorn, and the like. . . . In certain trees the bud first begins to grow within the seed itself, and, as it increases in size, the seeds split—for all such seeds are in a manner in two halves; and those of leguminous plants again all plainly have two valves and are double—and then the root is immediately thrust out; but in cereals, since the seeds are in one piece, this does not occur but the root grows a little before the bud.

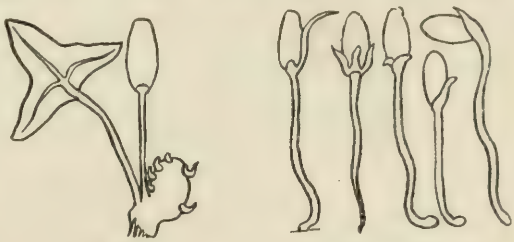


FIG. 37. Seedlings from the 'Syrian Garden' of Tethmosis III (about 1500 B.C.) at Karnak.[1]

'Barley and wheat come up with a single leaf, but peas, beans, and chick peas with several. All the leguminous plants have a single woody root, and also slender side roots springing from this . . . but wheat, barley, and the other cereals have a number of fine roots whereof they are matted together. . . . And there is a sort of contrast between these two classes; the leguminous plants which have a single root, have many side-growths above from the stem . . . while the cereals which have many roots, send up many shoots, but these have no side-shoots.'[2]

There can be no doubt that this is a piece of first-hand and minute observation of the behaviour of germinating seeds. The distinction between dicotyledons and monocotyledons is accurately

[1] These tracings were made from photographs kindly taken for the purpose by Captain Engelheart of the Department of Antiquities of the Egyptian Government. Cf. also Mariette-Bey's *Karnak*, Leipzig, 1875, Pl. xxxi.

[2] *Historia plantarum*, viii. 1, i.

set forth, though the stress is laid not so much on the cotyledonous character of the seed as on the relation of root and shoot. In the dicotyledons the root and shoot are represented as springing from the same point and in the monocotyledons from opposite poles in the seed.

No effective work was done on the germinating seed until the invention of the microscope, and the appearance of the work of Highmore (1613–85) [1] (Fig. 38), and the much more searching investigations of Malpighi (1628–94) [2] (Figs. 42 and 43) and Grew (1641–1712) [3] after the middle of the seventeenth century. The observations of Theophrastus are, however, so accurate, so lucid, and so complete that they might well be used as legends for the plates of these writers two thousand years after him.

Much has been written as to the knowledge of the sex of plants among the ancients. It may be stated that of the sexual elements of the flower no ancient writer had any clear idea. Nevertheless, sex is often attributed to plants, and the simile of the *Loves of Plants* enters into works of several of the poets, agricultural authors, and writers of fiction. Among these Achilles Tatius, the fifth-century author of the romance *The Adventures of Leucippe and Clitophon*, offers a good example.

> 'Plants', we there read, 'fall in love with one another and the palm is particularly susceptible. . . . If the female be planted at any considerable distance, the loving male begins to wither away. The gardener realizes what is the cause of the tree's grief, goes to some slight eminence in the ground, and observes in which direction it is drooping (for it always inclines towards the object of its passion) . . .' Then 'he takes a shoot of the female palm and grafts it into the very heart of the male. This refreshes the tree's spirit, and the trunk, which seemed on the point of death, revives and gains new vigour in joy at the embrace of the beloved; it is a kind of vegetable marriage.' [4]

Plants are frequently described as male and female in ancient biological writings, and Pliny goes so far as to say that some considered all herbs and trees were sexual.[5] Yet when such passages can be tested it will be found that these so-called males and females are usually different species. In a few cases a sterile

[1] Nathaniel Highmore, *A History of Generation*, London, 1651.

[2] Marcello Malpighi, *Anatome plantarum*, London, 1675.

[3] Nehemiah Grew, *Anatomy of Vegetables begun*, London, 1672.

[4] Achilles Tatius, i. 17.

[5] Pliny, *Naturalis historia*, xiii. 4.

variety is described as the male and a fertile as the female.[1] In a small residuum of cases dioecious plants are regarded as male and female, but with no real comprehension of the sexual nature of the flowers. There remains a minute group, which cannot be extended beyond the palms, in which the knowledge of plant sex had advanced a trifle further.

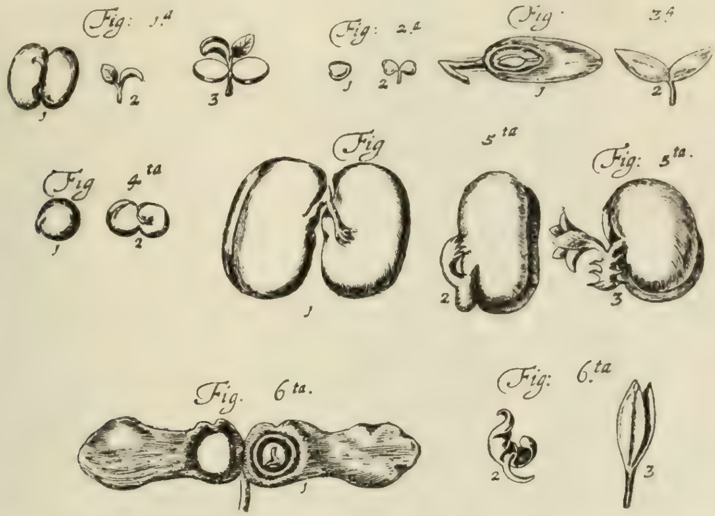


FIG. 38. GERMINATION OF SEEDS. From Nathaniel Highmore's *History of Generation*, London, 1651.

'The first figure, of the first Table, shews the Kidny Bean opened; in which is a little crooked leaf folded up, which being displayed, shews itself, as in the second; and when, being set, it arises above ground, it is such a Plant as the third shews, with the very same leaves and no other.

'The second figure shews a Colewort seed: the first shews both leaves, with the stalk folded up, as they lie in the husk of the seed: the second shews it come up out of the ground.

The Third Figure hath the small germen of an Ash; lying with his two leaves in the kernel of an Ash, both in the husk inclosing them. The second shews him sprung up above the Earth, at his first coming abroad.

'The fourth delineates the young germen of the Pease in the midst of the grain, and its breaking forth.

'The fifth shews the young Plant in the midst of the Bean: with the manner of his putting forth, with the same leaves displayed in the third, which are wrapt up in the first and second.

'The sixth Figure displayes the young Maple wrapt up in his husk; and how he lies, as in the first: The second shews him a little unfolded, when it is taken out of the husk. The third shews him gotten from his shell, and the surface of the Earth.'

'Common to all trees', Theophrastus tells us, 'is that by which men distinguish the "male" and the "female", the latter being fruit-bearing, the former barren in some kinds. In those

[1] A good collection of references to plant loves and plant sexes in classical writings can be found in R. J. Thornton's sumptuous *New illustrations of the sexual of Carolus von Linnaeus*, London, 1807.

kinds in which both forms are fruit-bearing, the "female" has fairer and more abundant fruit, however, some call these the "male" trees—for there are those who actually thus invert the names. This difference is of the same character as that which distinguishes the cultivated from the wild tree.'[1] The description

Figs. 39 and 40. Supernatural figures from Nineveh holding male inflorescence of date palm (from Layard).

by Theophrastus of the fertilization of the date palm is, however, quite clear. 'With dates it is helpful to bring the male to the female; for it is the male which causes the fruit to persist and ripen, and this process some call by analogy (!) *the use of the wild fruit.* The process is thus performed; when the male palm is in flower they at once cut off the spathe on which the flower is, just as it is, and shake the bloom with the flower and the dust over the fruit of the female, and, if this is done to it, it retains the fruit and does not shed it.'[2] The fertilizing character of the spathe of the male date palm was familiar in Babylon from a very early date, and is represented by a frequent symbol on the monuments in which a divine figure is represented as holding

[1] *Historia plantarum*, iii. 8, i. [2] *Historia plantarum*, ii. 8, iv.

the male inflorescence of the palm and fertilizing the female tree by shaking it (Figs. 39, 40, and 41).

The description of caprification by Theophrastus may here be appropriately related. He tells us that there are certain trees, the fig among them, ' which are apt to shed their fruit prematurely and remedies are sought for this. In the case of the fig the device adopted is caprification. Gall insects come out of the wild

FIG. 41. Assur-nasir-pal, King of Assyria, about 885–860 B.C., with winged attendants holding male inflorescence of date palm, performing ceremony of fertilization before conventionalized tree. Above is the symbol of the god Assur. From a bas-relief on the walls of the palace of Assur-nasir-pal, discovered at Calah (Nimrûd), now in the British Museum.

figs which are hanging there, eat the tops of the cultivated figs, and so make them swell '.[1] These gall-insects ' are engendered from the seeds. The proof given of this is that, when they come out, there are no seeds left in the fruit; and most of them in coming out leave a leg or wing behind. . . . A fig which has been subject to caprification is known by being red and parti-coloured and stout, while one which has not been so treated is pale and sickly '.[2] That Theophrastus had no idea whatever of the true nature of this caprification he shows a little farther on by observing that ' in the case both of the fig and of the date it appears that the " male " renders aid to the " female "—for the fruit-bearing tree is called female—but whilst in the case of the fig there is a union of the two sexes, in the case of the palm the result is brought about somewhat differently '.[3]

[1] *Historia plantarum*, ii. 8, i. [2] *Historia plantarum*, ii. 8, ii.
[3] *Historia plantarum*, ii. 8, iv.

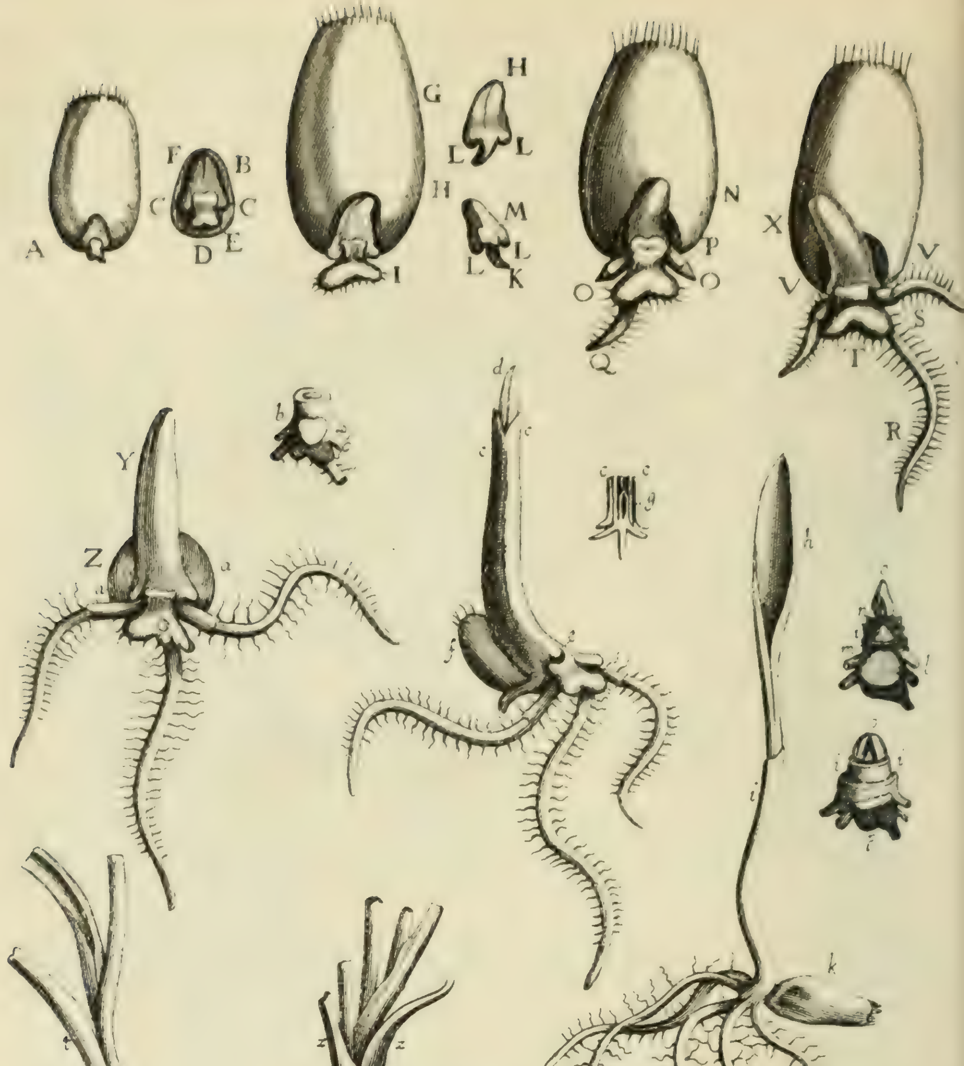



Fig. 42. THE GERMINATION OF WHEAT
From the *Anatome plantarum* of Malpighi, London, 1676.

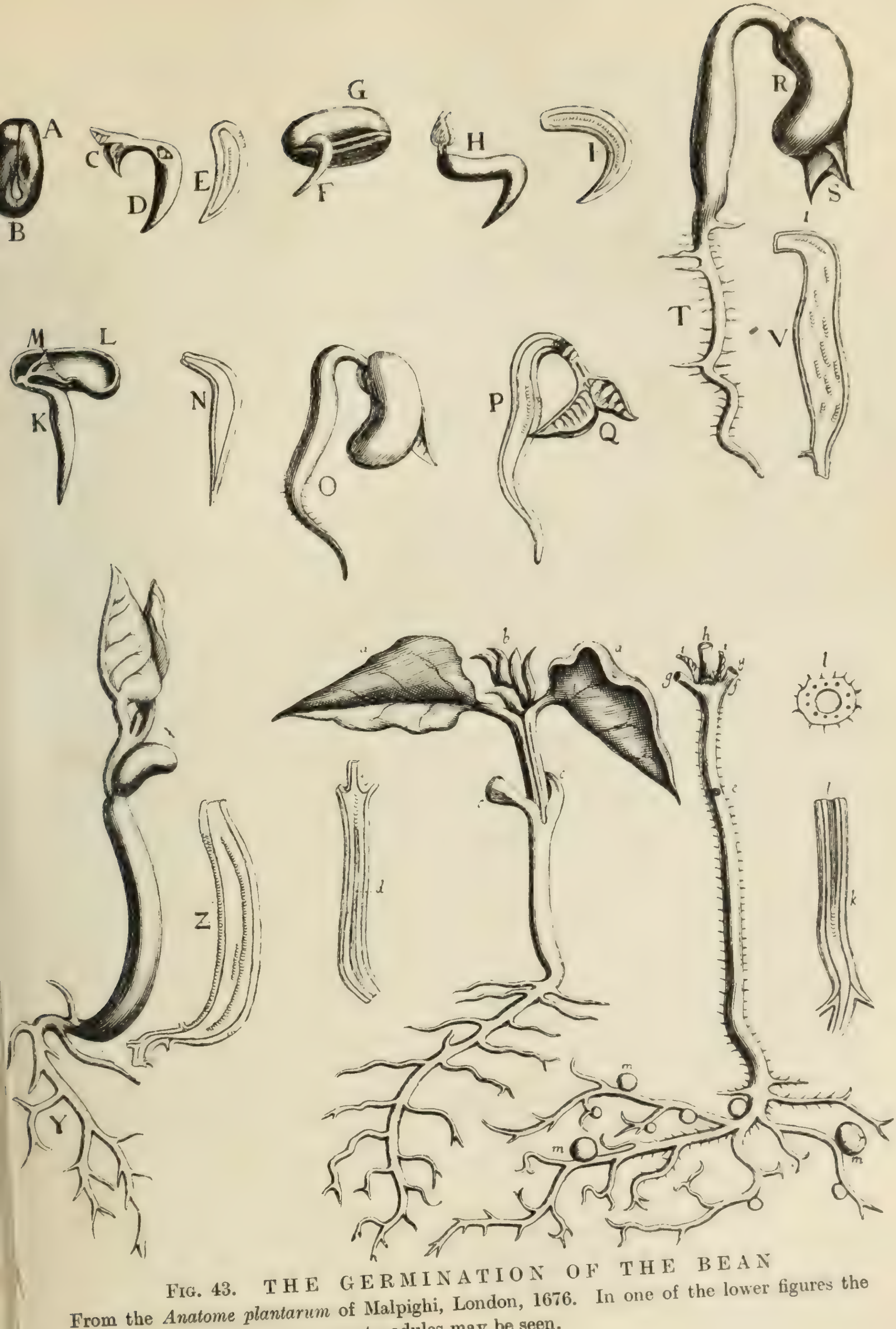



FIG. 43. THE GERMINATION OF THE BEAN
From the *Anatome plantarum* of Malpighi, London, 1676. In one of the lower figures the root nodules may be seen.

It is interesting to observe that Herodotus (about 500 B. C.), describing the fertilization of the date palm in Babylon, compares the process with that of the fig. 'The people of Babylon', says Herodotus, 'have date-palms growing over all the plain, most of them fruit-bearing, and to these they attend in the same manner as to fig-trees, and in particular they take the fruit of those palms which the Hellenes call male-palms, and tie them upon the date-bearing palms, so that their gall-fly may enter into the date and ripen it and that the fruit of the palm may not fall off; for the male palm produces gall-flies in its fruit just as the wild fig does.'[1]

Neither Theophrastus nor any ancient author ever saw the flowers of the fig. These were first distinguished by the youthful botanical genius Valerius Cordus in the first half of the sixteenth century.[2]

### (*c*) *Form and Structure of Plants*

Theophrastus was not successful in distinguishing the nature of the primary elements of plants, though he was able to separate root, stem, leaf, stipule, and flower on morphological as well as to a limited extent on physiological grounds. For the root he adopts the familiar definition, the only one possible before the rise of chemistry, that it 'is that by which the plant draws up nourishment',[3] but he shows by many examples that he is capable of following out its morphological homologies. Thus he knows that the ivy regularly puts forth roots from the shoots between the leaves, by means of which it gets hold of trees and walls,[4] that the mistletoe will not sprout except on the bark of living trees into which it strikes its roots, and that the very peculiar formation of the banyan tree is to be explained by the fact that 'this plant sends out roots from the shoots till it has hold on the ground and roots again: and so there comes to be a continuous circle of roots round the tree, not connected with the main stem but at a distance from it'.[5] He did not succeed, however, in distinguishing the real nature of such structures as bulbs, rhizomes, and tubers, but regards them all as roots. Nor was he more successful in his discussion of the nature of stems.

[1] Herodotus, i. 493.

[2] The passage in Valerius Cordus in which the flowers of the fig are described presents certain difficulties. It is discussed by E. L. Greene, *Landmarks of Botanical History*, Washington, 1909, p. 292.

[3] *Historia plantarum*, i. 1, ix.

[4] *Historia plantarum*, iii. 18, x.

[5] *De causis plantarum*, ii. 23.

As to leaves he is more definite and satisfactory though wholly in the dark as to their function. In speaking of them he is especially handicapped by the absence of a botanical nomenclature, so that his descriptions are seldom more than comparisons to well-known objects. ' Leaves ', he says, ' differ in their shape, some are round, as those of pear, some rather oblong, as those of apple ; some come to a sharp point and have spinous projections at the side, as those of smilax . . . some are divided and like a saw, as those of silver fir and of ferns. To a certain extent those of the vine are also divided, while those of the fig one might compare to a crow's foot. Some leaves again have notches, as those of elm, filbert, and oak, others have spinous projections both at the tip and at the edges, as those of kermes-oak, oak, smilax, bramble, *Paliouros* [Christ's thorn], and others. . . . Again there is the difference that some leaves have no leaf-stalk, as those of squill and purse-tassels, while others have a leaf-stalk. . . .' [1] He was well on the way, however, towards arriving at a correct idea of the nature of certain pinnate leaves. Thus of the mountain ash he says that ' the leaves grow attached to a long fibrous stalk, and project on each side in a row, like the feathers of a bird's wing, the whole forming a single leaf but being divided into lobes with divisions which extend to the rib ; but each pair are some distance apart, and when the leaves fall, these divisions do not drop separately but the whole wing-like structure drops at once.' [2] Again of the elder, ' the leaf is composed of leaflets growing about a single thick fibrous stalk to which they are attached at either side in pairs at each joint ; and they are separate from one another, while one is attached to the tip of the stalk ',[3] and of the Terebinth, ' the leaf is made up of a number of leaflets, like bay leaves, attached in pairs to a single leaf-stalk. So far it resembles the leaf of the [mountain ash], there is also the extra leaflet at the tip, but the leaf is more angular than that of the [mountain ash] and the edge resembles more the leaf of the bay '.[4] These passages on the nature of pinnate leaves are the more remarkable when we recall that they remained neglected until similar distinctions and observations were published by Johann Vaget in 1678 in his edition of the work of his master Joachim Jung (1587–1657).[5]

[1] *Historia plantarum*, i. 10, v, vi and vii.

[2] *Historia plantarum*, iii. 12, vi.

[3] *Historia plantarum*, iii. 13, v.

[4] *Historia plantarum*, iii. 15, iii.

[5] Joachim Jung, *Isagoge phytoscopica*, Hamburg, 1678.

In spite of his frequent use of the terms 'male and female' as applied to plants, Theophrastus, as we have already said, had no correct idea of the nature of sex in flowers. His description of flowers is thus almost entirely morphological. 'Some flowers', he says, 'are hair-like, as that of the vine . . . some are "leafy" as in almond, apple, pear, plum. Again some of these flowers are conspicuous, while that of the olive, though it is "leafy", is inconspicuous'. The flowers of annuals are usually, he says, 'two coloured and twofold. I mean by *twofold* that the plant has another flower inside the flower in the middle, as with rose, lily, violet. Some flowers again consist of a single leaf, [i.e. are gamopetalous or sepalous], having merely an indication of more, as that of bindweed. For in the flower of this the separate *leaves* are not distinct; nor is it so in the lower part of the narcissus, but there are angular projections from the edges. . . .' [1]

Notwithstanding his lack of insight as to the nature of sex in flowers, he attained to an approximately correct idea of the relation of flower and fruit. 'Some plants', he says, 'have the flower close above the fruit as vine and olive; in the latter, when the flowers drop off, they are seen to have a hole through them, and this men take for a sign whether the tree has blossomed well; for if the flower is burnt up or sodden, it sheds the fruit along with itself, and so there is no hole through it. The majority of flowers have the fruit case in the middle of them, or it may be the flower is on the top of the fruit case as in pomegranate, apple, pear, plum, and myrtle . . . for these have their seeds below, beneath the flower, and this is most obvious in the rose because of the size of the seed vessel. In some cases again the flower is on top of the actual seeds as in pine, thistle, safflower, and all thistle-like plants. . . . In some other plants the attachment is peculiar as in ivy and mulberry, and in these the flower is closely attached to the whole fruit-case.' [2] Thus Theophrastus, while never finally defining a flower, really comes gradually to abandon his first suggestion that a flower is but a whorl of specially coloured leaves, and almost comes to regard as the essential floral element its relation to the fruit. He has, moreover, succeeded in distinguishing between the hypogynous, perigynous, and epigynous types of flowers.

In spite of the discoveries of Valerius Cordus and the use of flowers for classification by de l'Obel and by Cesalpino in the

[1] *Historia plantarum*, i. 13, ii. [2] *Historia plantarum*, i. 13, iii.

sixteenth century, the sexual character of flowers remained very obscure for a hundred years after their time. Grew (1641–1712) in 1682 distinguished the stamens, or *attire* as he called them, from the outer floral whorls. He watched the anthers or *semets* bursting and scattering their pollen. Grew almost ignores the pistil, but 'in discourse with our learned *Savilian* Professor Sir Thomas Millington, he told me, he conceived, That the *Attire* doth serve, as the *Male*, for the *Generation* of the *Seed*. I immediately reply'd, That I was of the same opinion and gave him some reasons for it. . . . But withall, in regard every *Plant* is *Male* and *Female*, that I was also of opinion, That it serveth for the *Separation* of some *Parts* as well as the *Affusion* of others '.[1] Ray (1627–1705) spoke in somewhat similar indefinite terms,[2] and the sexual character of flowers was only cleared up by the work of Jacob Camerarius (1665–1721) in the last decade of the seventeenth century.[3]

### (*d*) *Habits and Distribution of Plants*

Theophrastus had a perfectly clear idea of plant distribution as dependent on soil and climate. ' Differences in situation and climate, he says, affect the result. In some places, as at Philippi, the soil seems to produce plants which resemble their parent ; on the other hand a few kinds in some few places seem to undergo a change, so that wild seed gives a cultivated form, or a poor form one actually better.' [4] These changes he acutely contrasts to the metamorphoses of animals.

' In pot-herbs ', he says, ' change is produced by cultivation ; for instance, they say that if celery seed is trodden and rolled in after sowing, it comes up curly ; it also varies from change of soil, like other things. . . . It would seem more surprising if such changes occurred in animals naturally and frequently ; some animals do indeed seem to change according to the seasons, for instance, the hawk, the hoopoe, and other similar birds. . . . Most obvious are certain changes in regard to the way in which animals are produced, and such changes run through a series of creatures ; thus a caterpillar changes into a chrysalis, and this in turn into the perfect insect. . . . But there is hardly anything abnormal in this, nor is the change in plants, which is the subject of our inquiry, analogous to it. That kind of change occurs in trees and

[1] Nehemiah Grew, *The Anatomy of Plants*, London, 1682, p. 171.

[2] John Ray, *The Wisdom of God manifested in the Works of the Creation*, London, 1691.

[3] R. J. Camerarius, *Ephem. Leopold. Carol. Acad.*, 1691, and *De sexu plantarum epistola*, Tübingen, 1694.

[4] *Historia plantarum*, ii. 2, vii.

in all woodland plants generally, as was said before, and its effect is that when a change of the required character occurs in the climatic conditions, a spontaneous change in the way of growth ensues.' [1]

He is very clear as to the difference between the vegetation of mountain and plain, and gives formal lists to illustrate it.[2]

'Again the character of the position makes a great difference as to fruit-bearing. The *persea* of Egypt bears fruit . . . but in Rhodes it only gets as far as flowering. The date-palm in the neighbourhood of Babylon is marvellously fruitful; in Hellas it does not even ripen its fruit, and in some places it does not even produce any.' 'That each tree seeks an appropriate position and climate is plain from the fact that some districts bear some trees but not others; the latter do not grow there of their own accord, nor can they easily be made to grow, and that even if they obtain a hold, they do not bear fruit. . . . Thus in Egypt there are a number of trees which are peculiar to that country, the sycamore, the tree called *persea* . . . and some others. Now the sycamore to a certain extent resembles the tree which bears that name in our country. Its leaf is similar, its size, and its general appearance; but it bears its fruit in a quite peculiar manner . . . not on the shoots or branches, but on the stem; in size it is as large as a fig.' [3]

At times Theophrastus seems to be on the point of passing from a statement of climatic distribution into one of real geographical regions. Thus:

'Among the plants that grow in Arabia, Syria, and India the aromatic plants are somewhat exceptional and distinct from the plants of other lands; for instance, frankincense, myrrh, cassia, balsam of Mecca, cinnamon, and all other such plants. . . . So in the parts towards the East and South there are these special plants and many others besides.

'In the northern regions it is not so, for nothing worthy of record is mentioned except the ordinary trees which love the cold and are found in our country.' [4]

The general question of plant distribution long remained at, if it did not recede from, the position where Theophrastus left it. The usefulness of the manuscript and early printed herbals was marred by the retention of plant descriptions prepared for the Greek East or Latin South, and these works were saved from complete ineffectiveness only by an occasional appeal to nature.

[1] *Historia plantarum*, ii. 4, vi. and vii.

[2] *Historia plantarum*, iii. 1, i.

[3] *Historia plantarum*, iii. 3, v.

[4] *Historia plantarum*, iv. 1, v.

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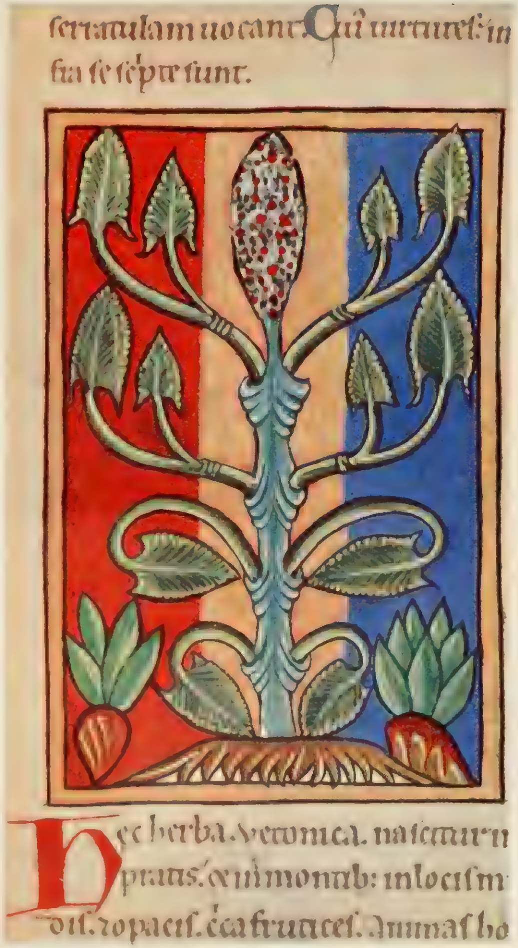


Early XIII Cent.

Cotton Vitellius C.III fo. 20r

About 1000

Harley 1585 fo. 14r

About 1200

Euricus Cordus (1486–1535) was perhaps the first to realize that there was only a general and not a specific correspondence between the plants of middle Europe and those of the Mediterranean region known to Dioscorides. With the extension of travel in the preparation of works on exotic botany such as those of Oviedo, Clusius and Acosta, this point gradually came into clearer view, but the conception of botanical regions was a very slow growth.

Theophrastus does not abandon the doctrine of plant metamorphosis and he applies this view especially to the transformation of corn into tares. 'They say', he says, 'that wheat and barley change into darnel, and especially wheat; and that this occurs with heavy rains and especially in well watered and rainy districts.' It is a fact that in damp fields the *Lolium* is liable to spring up, and this event, it is said, is still held by farmers to be of the nature of a metamorphosis. Theophrastus, though he does not deny that this is a metamorphic process, expresses his doubts. 'That darnel', he says, 'is not a plant of the spring like others (for some endeavour to make this out) is clear from the following consideration: it springs up and becomes noticeable directly winter comes; and it is distinguished in many ways; the foliage is narrow, abundant, and glossy.'[1]

In the case of the *Lolium* Theophrastus rightly expresses his disbelief in a dimorphism or transformation that we now know does not exist. There is another case in which a plant exhibits two stages of development that were almost invariably regarded by the ancients as separate species. The habit of the ivy is to develop a climbing shoot well provided with rootlets which hold it to its support. The climbing shoot is beset with dark angular foliage. After reaching the top of the object on which it is climbing there sets in an extensive production of free terminal branches bearing somewhat paler and larger ovate leaves. On these terminal twigs the flowers are born, and often the production of the terminal growth becomes so extensive as to conceal the climbing portion. In ancient times, as in the Middle Ages and indeed into quite modern times, these two stages in the development of the ivy were regarded as two species (Plate XXIII). The climbing plant was named by the Greek *helix* and the bushy terminal growth *cittus* or *chrysacanthus*. Theophrastus, however, ventures to traverse this view.

'The *helix*', he says, 'presents . . . differences in the leaves, which are small, angular, and of more graceful proportions, while

[1] *Historia plantarum*, viii. 7, i.

those of the *cittus* are rounder and simple; there is also difference in the length of the twigs, and further in the fact that this tree is barren. For as to the view that the helix by natural development turns into the *cittus* some assert that this is not so, the only true ivy according to these being that which was ivy from the first; whereas if, as some say, the helix invariably turns into [cittus], the difference would be merely one of age and condition and not of kind.'[1]

We may terminate our discussion with a summary of the botanical position of Theophrastus:

1. He distinguished the external organs of plants, naming them in regular sequence from root to fruit, and attained in many cases to a really philosophical distinction.
2. He definitely set forth the leaf homology of the perianth members of flowers but attained to no real knowledge of their sexual nature.
3. He established the first rudiments of a botanical nomenclature.
4. He watched the development of seeds and was able to some extent to distinguish between dicotyledons and monocotyledons.
5. He established a relationship between structure and habits and approaches the conception of geographical distribution.
6. He saw the need for a general classification of plants and made some attempt at a system though he failed to produce one which was in fact workable.
7. He perceived a general relation between structure and function in plants, and thus laid the basis of scientific botany.

Note.—I have to thank many friends for much kind help during the preparation of this essay.

Mrs. Agnes Arber and Professor D'Arcy Thompson read the work in manuscript and made a number of corrections and emendations.

Professor Sudhoff has most generously handed over to me a large collection of photographs of early herbals which must have taken years to put together. It is a source of great regret to me that these photographs reached me too late for full use in illustration of my text. I have, however, been able to include figures of the Leyden Apuleius and it is a pleasure to me to

[1] *Historia plantarum*, iii. 18, vii.

acknowledge that I owe my acquaintance with that important document and many others, including the frontispiece of this book, entirely to his good offices. In a later publication I hope to display more fully the rich vein of treasure which the generosity of this eminent scholar has placed at my disposal.

I am grateful to Mr. Henry Balfour, Dr. A. H. Church, Professor F. J. Cole, Professor Clifford Dobell, Dr. Claridge Druce, Professors Ernest and Percy Gardner, Mr. E. A. Lowe, Mr. Eric Maclagan, Mr. F. S. Marvin, Professor F. W. Oliver, Mr. C. Tate Regan, Mr. R. R. Steele, and Dr. W. J. Turrell for a number of suggestions that they have made.

For the loan of the blocks of Plates VI and VII illustrating the Julia Anicia MS. I have to thank Mrs. Arber and the Cambridge University Press. Plate XVIII of the Phillipps Dioscorides is from photographs taken for me by Mr. E. A. Lowe by kind permission of the then owner, Mr. Fitzroy Fenwick. The Paris Bibliothèque nationale MSS. lat. 6862 and gr. 2179 were examined for me by Miss A. Anderson who has helped me also with many details of the work. Miss C. Hugon has redrawn many of the text figures and has been of great assistance in the preparation of the plates.

In quoting passages from the works of Aristotle I have used the translations of Professor D'Arcy Thompson, Professor A. Platt, and the late Dr. Ogle in the *Oxford Aristotle*. The only deviation I have made has been the restoration, in a few cases, of the Greek term for that used by the translator. In every case I have given the reference by page and line to Bekker's Greek text. For the *History of Plants* of Theophrastus, the translation by Sir Arthur Hort has been followed. In one instance, however (*Historia plantarum*, i. 1, iv, p. 83 of my essay) I have ventured to diverge from it. To all these writers and to their publishers, the Clarendon Press, and the Loeb Classical Library, my thanks are due for permission to avail myself of these translations. There is no English version of the *De causis plantarum* and for that I have used Wimmer's text.

The Appendix contains a list of Aristotelian MSS. abstracted from the *Catalogue of Scientific and Medical MSS. in the British Isles* that is being prepared by my wife. She has helped me in innumerable ways and the revision of the proofs has been her work.

## APPENDIX

### MSS. IN ENGLISH LIBRARIES OF ARISTOTELIAN BIOLOGICAL WORKS

A list of the MSS. as they exist in the public libraries of this country is here given. For convenience of reference the Greek MSS. have been included in the list but not in the summary at the end. All MSS. are Latin unless otherwise described. Many give abridged versions of the works.

The time distribution of the MSS. is significant. It does not suggest any revived interest in Aristotelian biology with the revival of Greek learning in the fifteenth century. On the contrary, the number of Aristotelian biological MSS. of the fifteenth century is but a third of that of the fourteenth. The fall in numbers in the fifteenth century cannot be explained by the advent of printing, since the number of medical and scientific MSS. of almost every class of the fifteenth century is, in fact, greater than of the fourteenth century.

#### *Historia animalium*

| | | |
|---|---|---|
| 1. | British Museum: Royal 9 A XIV. Imperfect | 13th century. |
| 2. | British Museum: Royal 12 C XV. Translation by Michael Scot | 13th ,, |
| 3. | British Museum: Royal 12 F XV. Translation by Michael Scot | 13th ,, |
| 4. | Oxford: Balliol 250 | 13th ,, |
| 5. | Oxford: Balliol 252 | 13th ,, |
| 6. | Cambridge: Gon. & Caius 109. Translation by Michael Scot | 13th ,, |
| 7. | Cambridge: Peterhouse 121 | 13th ,, |
| 8. | Cambridge Univ. Lib. Ii. III. 16. Translation by Michael Scot | 13th ,, |
| 9. | Salisbury Cathedral 111 | 13th ,, |
| 10. | British Museum: Harley 4970 | 14th ,, |
| 11. | British Museum: Royal 7. C. I. Translation by Michael Scot | 14th ,, |
| 12. | Oxford: All Souls 72. Avicenna's paraphrase | 14th ,, |
| 13. | Oxford: Merton 278. Translation by Michael Scot | 14th ,, |
| 14. | Cambridge Univ. Lib. Dd. IV. 30. Translation by Michael Scot | 14th ,, |
| 15. | Oxford: Bod. Can. misc. 418 | ? early 15th, 14th ,, |
| 16. | Oxford: Bod. Barocci 95. Excerpts. GREEK | 15th ,, |
| 17. | Lincoln Cathedral B. 6. 4. Begins imperfectly | 15th ,, |

#### *De partibus animalium*

| | | |
|---|---|---|
| 1. | Oxford: C.C.C. 108. GREEK. Ends imperfectly | late 12th century. |
| 2. | British Museum: Royal 9 A XIV. | 13th ,, |
| 3. | British Museum: Royal 12. F. XV. Ends Imperfectly | 13th ,, |
| 4. | Cambridge: Peterhouse 121 | 13th ,, |
| 5. | Cambridge Univ. Lib. Ii. III. 16 | 13th ,, |
| 6. | British Museum: Harley 4970. | 14th ,, |
| 7. | British Museum: Royal 7. C. 1. Translation by Michael Scot | 14th ,, |
| 8. | Oxford: Merton 270 | 14th ,, |
| 9. | Oxford: Merton 271 | 14th ,, |
| 10. | Oxford: Bod. Can. misc. 412 | early 15th ,, |

#### *De generatione animalium*

| | | |
|---|---|---|
| 1. | Oxford: C.C.C. 108. GREEK | late 12th century. |
| 2. | British Museum: Royal 9. A. XIV | 13th ,, |
| 3. | Cambridge: Peterhouse 121 | 13th ,, |
| 4. | Cambridge Univ. Lib. Ii. III. 16 | 13th ,, |
| 5. | British Museum: Harley 4970 | 14th ,, |
| 6. | British Museum: Royal 7. C. I. Translation by Michael Scot | 14th ,, |
| 7. | Oxford: Merton 270 | 14th ,, |
| 8. | Oxford: Merton 271 | 14th ,, |
| 9. | Oxford: Bod. Can. misc. 412 | early 15th ,, |
| 10. | Oxford: New Coll. 226 | 15th ,, |

### *De incessu animalium*

| | | |
|---|---|---|
| 1. | Oxford: C.C.C. 108. GREEK | late 12th century. |
| 2. | Oxford: Balliol 250 | 13th ,, |
| 3. | Cambridge: Peterhouse 121 | 13th ,, |
| 4. | Cambridge: Peterhouse 190 | 13th ,, |
| 5. | Cambridge: Fitzwilliam. 155 | 13th ,, |
| 6. | Oxford: Balliol 232 A | 14th ,, |
| 7. | Oxford: Trinity 67. | 14th ,, |
| 8. | Oxford: Merton 271 | 14th ,, |
| 9. | Cambridge Univ. Lib. Ii. II. 10. | 14th ,, |
| 10. | Oxford: Bod. Can. misc. 418 | early 15th ,, |
| 11. | Oxford: New Coll. 226. GREEK | 15th ,, |
| 12. | Cambridge Univ. Lib. Mm. III. 11. Extracts | 15th ,, |

### *De motu animalium*

| | | |
|---|---|---|
| 1. | Oxford: Balliol 250 | 13th century. |
| 2. | Oxford: Balliol 250. Fragment | 13th ,, |
| 3. | Cambridge: Peterhouse 12 | 13th ,, |
| 4. | Cambridge: Fitzwilliam 154 | 13th ,, |
| 5. | Cambridge: Fitzwilliam 155 | 13th ,, |
| 6. | British Museum Add. 19582 | 14th ,, |
| 7. | Oxford: Bod. Can. Lat. auct. 290 | 14th ,, |
| 8. | Oxford: Merton 270 | 14th ,, |
| 9. | Oxford: Balliol 232 A | 14th ,, |
| 10. | Oxford: Trinity 67. | 14th ,, |
| 11. | Cambridge Univ. Lib. Ii. II. 10 | 14th ,, |
| 12. | Oxford: Bod. Can. misc. 412 | 15th ,, |
| 13. | Oxford: Bod. Digby 44. Extracts | 15th ,, |
| 14. | Oxford: New Coll. 226. GREEK | 15th ,, |
| 15. | Cambridge Univ. Lib. Mm. III. 11. Extracts | 15th ,, |

### *De plantis*

| | | |
|---|---|---|
| 1. | Oxford: Bod. Auct. F. 5. 31 | 12th century. |
| 2. | Oxford: C.C.C. 114. | 13th ,, |
| 3. | Cambridge: Gon. & Caius 409. Fragment | 13th ,, |
| 4. | Cambridge: Gon. & Caius 452 | 13th ,, |
| 5. | Cambridge: Gon. & Caius 506 | 13th ,, |
| 6. | Cambridge: Fitzwilliam 154 | 13th ,, |
| 7. | British Museum: Harley 3487 | 14th ,, |
| 8. | British Museum: Add. 19582 | 14th ,, |
| 9. | Oxford: Bod. Can. Lat. auct. 291 | 14th ,, |
| 10. | Oxford: Balliol 232 A | 14th ,, |
| 11. | Oxford: C.C.C. 111. | 14th ,, |
| 12. | Cambridge Univ. Lib. Ii. II. 10 | 14th ,, |
| 13. | Durham Cathedral C. III. 17 | 14th ,, |
| 14. | Durham Cathedral C. IV. 18 | 14th ,, |
| 15. | British Museum: Sloane 2459 | 15th ,, |
| 16. | Oxford: Bod. Bodley 675 | 15th ,, |
| 17. | Oxford: C.C.C. 113. GREEK | 15th ,, |
| 18. | Cambridge: Peterhouse 184 | 15th ,, |

### SUMMARY OF LATIN TEXTS

| | No. of MSS. |
|---|---|
| 12th century | 1 |
| 13th ,, | 30 |
| 14th ,, | 32 |
| 15th ,, | 12 |

# MEDIAEVAL ASTRONOMY

By J. L. E. Dreyer

The study of Mediaeval Cosmology and Astronomy has hitherto not attracted many students. This is perhaps partly due to the fact that the period in question is one of stagnation, during which astronomy made absolutely no progress in Christian countries, while the high state reached by science at Alexandria had gradually to be won back. But the chief reason of the neglect is that many interesting writings from the Middle Ages have never been printed and have therefore to be looked for among the manuscript treasures in great libraries, particularly in the Bibliothèque Nationale at Paris. The great work of M. Pierre Duhem, *Le Système du Monde, Histoire des Doctrines cosmologiques de Platon à Copernic*,[1] is therefore particularly welcome, and it is quite up to the high standard of excellence of his previously published historical works, *Études sur Léonard de Vinci* and *Les Origines de la Statique.* So far, five volumes of more than five hundred pages each have been published, and it is remarkable that so great a work should have appeared in France during the terrible struggle in which that country was then involved. The five volumes reach to the beginning of the fifteenth century; and how far the work will be continued may be doubtful, as the death of the author was announced in 1916. It was stated in the Paris Academy in December 1916 [2] that the work was to have been completed in ten volumes, and that the fifth and sixth had been entrusted by M. Duhem's daughter to the Academy. The fifth volume appeared in 1917. But even if not completed according to the original plan, the work will be of exceptional interest on account of the great number of manuscripts which M. Duhem has examined, and from which he has given lengthy extracts.

The work is (so far) divided into three parts. 'Greek Cosmology' occupies the first volume and four-fifths of the second

[1] Paris, A. Hermann et Fils, 5 vols., 1913–17.

[2] *Comptes rendus*, December 18, 1916.

one. 'Latin Astronomy in the Middle Ages' reaches to the middle of the fourth volume, and is followed by 'The rise of Aristotelism', which at the end of the fifth volume is carried as far as Thomas Aquinas. As Greek Cosmology has been dealt with in two works published in England during the last fifteen years, that part of M. Duhem's work does not call for special notice in this place. Neither do the chapters of Part I dealing with Arabian astronomy (which the author considers as a mere continuation of Greek science) contain anything new. As a rule, the author shows a thorough acquaintance with the literature of his subject, though we have in a few cases failed to find references to important works, such as the *Liber Jesod Olam* of Isaac Israeli or *Le livre de l'Ascension de l'esprit* of Abu'l Faraj. It is particularly the chapters dealing with Latin Astronomy in the Middle Ages which will be of permanent value, as they give accounts of many manuscript treatises never before described.

The last great astronomer of the Alexandrian school, Claudius Ptolemy (about A. D. 140), wrote a complete compendium of ancient astronomy as finally developed by Hipparchus and himself. During the 270 years which had elapsed since the days of Hipparchus astronomy had certainly not stood still, but we know next to nothing about the progress made, as Ptolemy gives very little historical information. The details given by Pliny about the situations of the apsides of the excentric orbits of the planets show, however, that Ptolemy had more than the work of Hipparchus to build on. Pliny's source was no doubt the book *De novem disciplinis* by M. Terentius Varro, which is unfortunately lost. It seems to have been a sort of condensed encyclopaedia, and was superseded by writings of a similar kind from the fourth and fifth centuries which have come down to us. These are, the commentary to Plato's *Timaeus* by Chalcidius, the commentary to Cicero's *Somnium Scipionis* by Macrobius, and the encyclopaedic book *De nuptiis Philologiae et Mercurii* by Martianus Capella. Being written in Latin they were more readily accessible to Western readers than the lengthy Greek works of Proklus and Simplicius ; and during the first half of the Middle Ages they were, together with Pliny's *Natural History*, the only books from which some knowledge of Greek science might be derived by students in the West.

The first feeble light after the dark night of the patristic

writers came from Isidore, Bishop of Seville, who died in 636. When dealing with dangerous topics such as the figure of the world and the earth he does not lay down the law himself, but quotes ‘ the philosophers ’ as teaching this or that, though without finding fault with them. In this manner he repeatedly mentions that heaven is a sphere rotating round an axis and having the spherical earth in its centre. The water above the firmament mentioned in the first chapter of Genesis had of course to be brought in, and Isidore states that the Creator tempered the nature of heaven with water, lest the conflagration of the upper fire should kindle the lower elements. Isidore gives as his authorities Hyginus (author of a versified description of the constellations), Clement of Alexandria, and the patristic writers, but does not mention Pliny, so that it is no wonder that his knowledge is very fragmentary.

The Venerable Bede, who lived a century later (he died about 735), was better informed. The contents of his treatise *De Natura Rerum* are taken from Pliny, often almost verbatim ; and the spherical form of the earth, the order of the seven planets circling round it, the sun being much larger than the earth, and similar facts are plainly stated. But the water around the heaven and the usual explanation of its existence could not be kept out of the book, even though Pliny did not mention it and though Bede had stated that the earth was a sphere. Another and much larger book on chronology (*De Temporum Ratione*) shows a fair knowledge of the annual motion of the sun and the other principal celestial phenomena. It is deserving of notice that Bede from his study of Pliny and from personal observation knew a good deal about the tides, and was the first to show that the ‘ establishment ’ of a port (or the mean interval between the time of high water and the time of the moon’s previous meridian passage) is different for different ports. But the sphericity of the earth was still rather unpopular among ecclesiastics, and even in the first half of the ninth century Hrabanus Maurus, Archbishop of Mainz, thought it best to say nothing about it. He merely says that the earth is in the middle of the world, and tries hard to reconcile the roundness of the horizon with the four corners of the earth alluded to in Scripture. His statement that the heaven has two doors, east and west, through which the sun passes, also looks as if his point of view was much the same as that of the patristic writers.

But he was the last prominent author of whom this may be said, and from about the ninth century the spherical figure of the earth and the geocentric system of planetary motions were reinstated in the places they had held as facts ascertained with certainty among Greek philosophers twelve hundred years earlier.

Among the writers of the ninth century who paid any attention to the construction of the Universe, the most remarkable was John Scotus Erigena. In his great work *De Divisione Naturae* he shows that he is acquainted with Chalcidius and Martianus Capella, and for the first time we perceive a very curious influence which these rather inferior writers exercised throughout the Middle Ages. In the fourth century B. C. Herakleides of Pontus, struck with the fact that Mercury and Venus are never seen at a great distance from the sun, had come to the conclusion that these two planets move, not round the earth, as the sun and the other planets were supposed to do, but round the sun, so that they are sometimes nearer to us and sometimes farther off than the sun. But this idea was coldly received; it was quite ignored by Ptolemy and is only mentioned by Theon of Smyrna and Macrobius (without alluding to Herakleides), and by Martianus Capella and Chalcidius, who give the credit to Herakleides. Theon was not known in the Middle Ages, but the three other writers were held in high repute; and this led to the planetary system described by them being known to many mediaeval writers, though to most of them rather confusedly, as if they did not quite understand it. Thus Erigena says: 'As to the planets which move round the sun, they show different colours according to the quality of the regions which they traverse; I speak of Jupiter, Mars, Venus, and Mercury, which incessantly circle round the sun, as Plato teaches in the *Timaeus*. When these planets are above the sun, they show us clear aspects, they look red when they are below it.' Plato says nothing at all about this; but perhaps Erigena had only read Chalcidius and assumed that what he said was also to be found in the *Timaeus*. Chalcidius only mentions Venus as moving round the sun, but as he had already described the apparent motions of the two inferior planets, he probably made no distinction between them. M. Duhem seems to consider it highly creditable to Erigena that he extended the system of Herakleides to Mars and Jupiter, which nobody else did for fully seven hundred years, till Copernicus and Tycho Brahe let all the five planets

move round the sun. But with regard to Jupiter, it is simply absurd to imagine that it is sometimes above the sun (i. e. more distant than the sun), sometimes below it (or nearer). As to Mars, that planet is certainly at opposition nearer to the earth than the sun is, but there is no reason to think that the astronomical knowledge of Erigena included that fact, as it is in other directions scanty enough and is confined to carelessly copied scraps from his few authorities. We need only mention his statement that half the circumference of the earth is equal to its diameter!

Probably also from the ninth century is another book about the Universe, *De mundi caelestis terrestrisque constitutione liber*, formerly ascribed to Bede, but quite certainly of much later date, since there are several allusions to the chronicles of Charlemagne. The author has a fair knowledge of the general celestial phenomena such as could be gathered from the above-mentioned sources, but no more. It is interesting to see that he favours the old idea sometimes met with among the Greeks, that the planets do not really travel from west to east, but from east to west, only more slowly than the sphere of the fixed stars do; so that Saturn, which comes to the meridian about eight seconds later every night, is the quickest planet, and the moon, which takes fully three-quarters of an hour longer than the fixed stars do, is the slowest—contrary to the usual idea, that Saturn, which takes 29½ years to go round the heavens in its orbit, is the slowest planet, and the moon, going round the heavens in 27 days, is the fastest.[1] We shall see presently that this primitive idea obtained many adherents towards the end of the Middle Ages. As to Mercury and Venus, the writer's opinion is, that they are sometimes above the sun and sometimes below it, as it is recorded in the *Historia Caroli* that Mercury was visible for nine days as a spot on the sun, though clouds prevented both the ingress and the egress being seen. But he does not say that they move in orbits round the sun. The writer shows himself somewhat independent of his authorities by adding a good deal of astrology and suggesting various rationalistic theories about the unavoidable 'supercelestial waters'.

Passing over the extremely elementary *Imago Mundi* of

[1] See for instance Plato, *Timaeus*, pp. 38–9, Leg. 821 sq.

doubtful age and authorship, we must next mention another work formerly counted among the writings of Bede, entitled Περὶ διδάξεων *sive elementorum philosophiae libri* IV. It was written by William of Conches, a Norman of the first half of the twelfth century.[1] It is strange that it should ever have been attributed to Bede, as it shows a freedom of thought which would have been impossible early in the eighth century. But his astronomical knowledge is often confused and erroneous. For instance, he knows that the orbit of the sun is a circle excentric to the earth, but he imagines that the great heat in summer is caused by the sun being at that time nearer to the earth than in winter. He is aware of the difference of opinion among the ancients as to the position of the solar orbit, whether it was just outside the lunar orbit (according to the Pythagoreans, Plato, Eudoxus, and Aristotle), or between the orbits of Venus and Mars, as taught by Archimedes and all subsequent writers, including Ptolemy. William of Conches thinks that this difference of opinion is caused by the fact that the periods of Mercury, Venus, and the sun are nearly equal, so that their circles must also be nearly equal and therefore are not contained one within the other but intersect each other. He therefore did not grasp the real meaning of the system of Herakleides, but merely conceived the three orbits to be nearly equal in size with their centres at short distances from each other and in a line with the earth; and his description agrees with a diagram given in an anonymous manuscript of the fourteenth century, copied by M. Duhem.

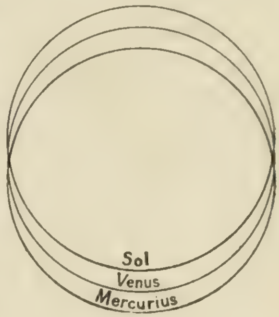



FIG. 1.

The question of the orbits of Mercury and Venus continued to crop up now and then, as long as Chalcidius and other late authors continued to be considered as great authorities by some writers who were much behind their own time. The Rabbi Abraham ben Ezra of Toledo (1119–75) in several of his astrological writings, which were printed in 1507 at Venice in a Latin translation, alludes to the orbits of Mercury and Venus being between those of the moon and the sun; but in one place he says

[1] Two manuscripts in the Bibliothèque Nationale give the author's name as William of Conches, and there are other proofs from other undoubted writings of his.

that the two planets are sometimes above and sometimes below the sun. The same expression is used in the following century by Bartholomaeus Anglicus in his encyclopaedic work *De proprietatibus rerum* (c. 1275). The only time that it is clearly and distinctly stated that Mercury and Venus travel round the Sun is in an astrological manuscript in the Bibliothèque Nationale of the year 1270 by an anonymous astrologer to the last Latin Emperor at Constantinople, Baldwin of Courtenay. After saying that the orbits of moon, sun, and three outer planets surround the earth, he continues: 'Li cercles de Vénus et de Mercure ne l'environent mie. Ainz corent environ le Soloil et ont lor centre de lor cercles el cors del Soloil; mes Mercurius a le centre de son cercle el milieu del cors del Soloil, Vénus l'a en la souraineté del cors del Soloil; et por ce sunt il dit épicercle, qu'il n'environent mie la terre, si cum j'ai dit desus des autres.' The author might have lived hundreds of years earlier; he knows nothing of Ptolemy or of the Arab writers on Ptolemaic astronomy, who long before the time he wrote had become known in the west of Europe.

For while Europe had been content to pick up a few crumbs here and there, the East had been feasting on the intellectual repast left by the Greeks. Works on Philosophy and Science had been translated into Arabic, and Mohammedan authors had written text-books founded on them and had continued the work of the Greeks in Mathematics and Astronomy. Arabic authors began to be known in the West from about the year 1000; Gerbert (Pope Sylvester II) probably wrote a book on the astrolabe founded on Arabic writings, and several tracts on the same subject were written in the eleventh and twelfth centuries in France, especially at Chartres, at that time the principal seat of learning there. Translations were also made in Italy by Plato of Tivoli; but it was in Spain, where science was still under the protection of powerful Arabian kings, that the work of translation was chiefly carried on. The first translations were the work of a college of interpreters established at Toledo; an Arabian scholar translating a book into the vulgar tongue, and a Spaniard afterwards turning this into Latin. Ptolemaic astronomy became known about the middle of the twelfth century through the medium of the books of Al Battani and Al Fargani. The original work of Ptolemy, the *Syntaxis* or the *Almagest*, as it was generally called in Latin countries from a corruption of part of the Arabic title

(Al-$\mu\epsilon\gamma\acute{\iota}\sigma\tau\eta$), was first translated about the same time by Gherardo of Cremona, who died about 1184 at the age of 73. He seems to have spent most of his life at Toledo, where he went to find the Almagest. Seeing what a great number of valuable works in Arabic were to be found there, he learned Arabic and is said to have translated no less than seventy-four different works, both by Greek and Arabian authors. But it took a very long time before people could be found capable of mastering the great work of Ptolemy.

That there were some people in the middle of the twelfth century anxious to spread knowledge of astronomy may be seen from a manuscript in the Bibliothèque Nationale, examined by M. Duhem. The name of the author of the 'Tables of Marseilles' is not known; from internal evidence it appears that they were prepared about the year 1140. The author says that students of astronomy were compelled to have recourse to worthless writings going under the name of Ptolemy and therefore blindly followed; that the heavens were never examined, and that any phenomena not agreeing with such books were simply denied. He therefore decided to transform the astronomical tables of Al Zarkali, which were computed for the meridian of Toledo and adapted to Arab years, so as to arrange them for the meridian of his native city and according to years dated from the birth of our Lord. This attempt to make the Toledo tables known in Latin countries did not bear fruit immediately; but early in the thirteenth century imitations of the tables of Marseilles began to appear, adapted to the meridians of Paris, London, Pisa, and Palermo, even for that of Constantinople, at that time ruled by Latin Emperors. The London tables date from the year 1232; the author mentions Ptolemy, but evidently only knows his work by name.[1] This is certainly also the case with the celebrated little book on the Sphere by John of Holywood (Joh. de Sacrobosco), written in the first half of the twelfth century, which continued to be a favourite text-book for three hundred years and was repeatedly printed. He only had his wisdom from Al Fargani and Al Battani, for he copies a mistake made by them and omits what they omit.

[1] Similar tables, founded on those of Al Zarkali, were made at Montpellier towards the end of the thirteenth century by the Jew Jacob ben Makir, generally called Profatius.

But astronomical books were far from being the only ones transmitted through the Arabs. The philosophical books of Aristotle and of his commentators, as well as neoplatonic and Arab speculations, also crossed the Pyrenees. At first they were not welcomed by the Church, and at a provincial council held at Paris in 1209 it was decreed that neither Aristotle's books on natural philosophy nor commentaries on them should be read either publicly or privately in Paris. In 1215 this prohibition was renewed in the statutes of the University of Paris. But this resistance wore off by degrees; better translations both of Aristotle and of Arab astronomers were produced by Michael Scot; while Guillaume d'Auvergne, Bishop of Paris from 1228 (died 1248), lent his powerful aid to the spreading of knowledge. He was a prolific writer, and was the first to make serious use of Greek and Arab philosophy, rejecting what was contrary to the Christian faith and combining the rest with what the Church taught, to compose a philosophical system acceptable to the Christian world. Among his writings was a treatise *De Universo*, which stands half way between the old works of Isidore, Bede, Pseudo-Bede, Honorius, and the later encyclopaedias of Albertus Magnus and Vincent of Beauvais, containing more philosophy and less theology than the former. But his opinions on celestial motions are very confused. For instance, he thinks that one can 'by means of astronomical instruments and certain geometrical instruments' determine the distance of the earth from each of the fixed stars and from each of the planets. The waters above the moving spheres are neither fluid nor in a state of vapour; they form an ethereal mass, perfectly transparent and immobile, separating those spheres from the Empyrean.

The introduction of Aristotelian natural philosophy in the Universities of Paris and Oxford brought about a prolonged strife between Aristotelian ideas of the construction of the Universe and the Ptolemaic system of the world; or rather a revival in France and England of an old dispute which had existed first in the Hellenistic and then in the Mohammedan world. Aristotle had adopted the 'homocentric spheres' of Eudoxus to account for the motions of the planets; but though this would to some extent explain the chief irregularities in these motions, continued observations soon showed that the system was insufficient to 'save the phenomena', particularly as it could not account for

the variable distance of a planet from the earth. A totally different system had therefore been developed at Alexandria in the course of nearly four hundred years, until it was completed by Ptolemy. According to this, a planet moved on the circumference of a circle (the epicycle), the centre of which travelled on a larger circle (the excentric or deferent) the centre of which was at some distance from the earth; but in such a manner that its motion was uniform, not with regard to the centre of the deferent, but as seen from another point, the *punctum aequans*. The centre of the deferent was midway between that point and the earth. Further complications had to be introduced to account for the motion in latitude of the planets. From a mathematical point of view this system was perfect, as it really could 'save the phenomena', that is, represent the actually observed motions with an accuracy nearly corresponding to that attainable by the crude instruments then in use. But it was totally at variance with Aristotelian Physics, the adherents of which viewed the movements around points outside the centre of the world with extreme disfavour. Long before Ptolemy's time attempts had therefore been made to reconcile the two systems. This was simple enough, as long as the deferent was assumed concentric with the earth. The epicycle might then be conceived to be the equator of a solid sphere, rolling between two solid concentric spheres. This idea is described by Theon of Smyrna (soon after A. D. 100), but it must be much older. It became untenable, as soon as the deferents became excentric circles. In the *Syntaxis* Ptolemy merely alludes to planetary spheres when describing the order of the various orbits (ix. 1); and his attitude with regard to the equivalence of the epicyclic and excentric theories shows that he had broken with the idea described by Theon and did not attribute any reality to the multiple motions of the *Syntaxis*, but merely considered them as geometrical means of representing the real motions. But in a later work, *Hypotheses of the Planets*, or rather in the second book of it, Ptolemy's ideas are quite different.[1] Here he proposes to do for the complicated theories of the *Syntaxis* what the system of Theon did for the simple epicyclic motion, producing not a mere model but a real representation of the constitution of the universe,

[1] The Greek original is lost, but a translation into Arabic has been preserved, from which a German translation was printed in 1907 (*Claudii Ptolemaei Opera*, ed. Heiberg, T. ii).

as real as that described in Aristotle's *Metaphysics*. The epicycle-sphere now fits between two excentric spherical surfaces which touch two other surfaces (an inner and an outer one), in the common centre of which the earth is situated. This system of the world does not seem to have been a success; in the neoplatonic schools the theories of the *Syntaxis* appear to have been more valued, although the old Platonic and Aristotelian dogma, that every celestial motion must be circular and uniform round the centre of the earth, still found partisans.

Towards the end of the eighth century Mohammedan nations began to become acquainted with Alexandrian astronomy, in the first instance through the medium of northern India, where a knowledge of Greek science had spread in the first couple of centuries after the conquests of Alexander the Great.[1] The system of spheres seems to have appealed strongly to Eastern minds; and throughout the time when astronomy continued to be successfully cultivated in the Mohammedan world we find that various combinations of spheres were proposed by people who could not be satisfied with the Ptolemaic system of circles, while the latter was accepted and used by professional astronomers. The first to describe the spheres was Tâbit ben Korrah, in the second half of the ninth century. He seems to have been the first to fix the number of spheres at nine, and he was followed by the 'Brethren of Purity' in the tenth century and by Ibn al Haitham (*c.* A. D. 1000).[2] The necessity of introducing a ninth sphere above the eighth sphere (the sphere of the fixed stars) was due to the imaginary phenomenon of trepidation or oscillatory movement of the equinoxes. This dates back to the time before Ptolemy, but he quite ignored it and taught that the Precession of the Equinoxes is uniformly progressive, while Tâbit (though speaking with a certain reservation) accepted the phenomenon of trepidation as real. The Arabian combinations of spheres were mainly borrowed from Ptolemy, though with modifications. It

[1] From what M. Duhem says (vol. ii, p. 213) it looks as if he thought that Arabian astronomy was founded on indigenous Indian knowledge. But it is quite certain that the Indians derived all their knowledge of planetary motion from the Greeks. See J. Burgess, in *Journal of the R. Asiatic Society*, October 1893, pp. 746 sq., and Dreyer, *Hist. of the Planetary Systems* (Cambridge, 1906), pp. 240 sqq.

[2] Known in the West as Alhazen, author of a celebrated book on Optics.

was particularly in Spain that the opposition to the Ptolemaic system of excentrics and epicycles came to the front, being intimately connected with the rapid rise of Aristotelian philosophy in that country in the twelfth century, which culminated in the work of Averroes, the greatest philosopher of Islam. An ingenious attempt at reviving the principle of homocentric spheres in a perfectly novel manner was made by the astronomer Al Bitrugi (Alpetragius), though the leading idea was probably due to the philosopher Ibn Tofeil.

This system of homocentric spheres differs from that of Eudoxus and Aristotle by assuming that the prime mover (the ninth sphere) everywhere produces only a motion from east to west, the independent motion of the planets from west to east being rejected. We have already mentioned that this was a very old idea which had been revived in Europe by Pseudo-Bede. But Al Betrugi saw that this was not sufficient, as not only is the pole of the ecliptic different from that of the equator, but the planets do not even keep at the same distance from the pole of the ecliptic but have each their motion in latitude as well as a variable velocity in longitude, all of which had to be accounted for. This is done by letting the pole of each planet's orbit describe a small circle round a mean position (the pole of the ecliptic) in the synodic period of the planet.[1] But the system was not worked out in detail; and only philosophers who wanted nothing more than a representation of the principal phenomena could be satisfied with it. In the eyes of astronomers it had many faults, the greatest being (as in the case of the system of Eudoxus) that it assumed a planet to be always at the same distance from the earth.

When Michael Scot about the year 1230 had produced his translations, the attacks of Averroes and Al Betrugi on the epicyclic system spread rapidly among the Scholastics. Though people who only desired to account for the apparent motions of the planets as seen projected on the celestial vault continued to follow the rules of Ptolemy, philosophers were greatly concerned

[1] The account given by M. Duhem of this historically important system is most unsatisfactory. For further details see *Hist. of the Planetary Systems*, p. 265, a book which seems to be unknown to M. Duhem. It is curious to see how astronomical historians have fought shy of explaining the system; see e. g. Delambre, *Hist. de l'Astr. du Moyen Age*, p. 174.

about the contradiction between Aristotle and Ptolemy. During the whole of the second half of the thirteenth century this agitated the two rival orders of Dominicans and Franciscans, who dominated the University of Paris. Among the former Albertus Magnus was at first the most prominent. He was much attracted by the system of Al Bitrugi, which he thought was very simple, because he ignored the small circles and thereby made it quite useless. It was in this simplified form only that the system continued to be known to most of the Scholastics, which sufficiently characterizes their superficial knowledge of celestial motions. Yet Albert was quite aware of the fatal objection to every form of homocentric system, and he finally declared for Ptolemy. The same was the case with his disciple Thomas Aquinas. For the simplified system of Al Bitrugi he substituted the idea that celestial bodies are animated by two movements ; the first is a uniform rotation from east to west, a principle of eternal duration ; the second is a rotation from west to east round the poles of the ecliptic, a principle of generation and transformation, a movement in which the different orbs take part to a lesser extent the more noble they are. Like Albert he ends by abandoning Averroes for Ptolemy, chiefly on account of the change of distance.

If we turn to the Franciscans we are met with more hesitation as to the choice between the rival theories. Bonaventura, the Doctor seraphicus, does not seem to have devoted much attention to astronomy. According to him there is a ninth heaven, the aqueous one, which is the primum mobile ; some philosophers have perceived that the firmament has a proper motion of 1° in a hundred years,[1] but whether this is true or not it is certain that Doctors of Theology admit that there is a moving heaven without stars.

But in Roger Bacon we meet at last with a man who was thoroughly acquainted with the astronomical writings both of Greeks and Arabs. At Oxford he was under the influence of Robert Grosse-Teste, afterwards Bishop of Lincoln, who had devoted some attention to astronomy and was a follower of Ptolemy, except when he wanted to be a metaphysician and had to follow Al Bitrugi. But it was not till Bacon went to Paris (about 1235) that he was able to study scientific problems seriously. M. Duhem gives a lengthy account of a manuscript

[1] The amount of precession according to Ptolemy.

in the municipal library of Amiens, containing several series of questions on the *Physics* and *Metaphysics* of Aristotle. They are probably written by pupils of Bacon at Paris, at latest about the year 1250. The questions on the *Metaphysics* must be the earliest ; the teacher does not appear to know Ptolemy's works, and only to have heard of Al Bitrugi. The questions on Aristotle's *Physics* show more knowledge, especially of the system of Eudoxus and of Precession. The subsequent writings of Bacon show how he persevered in his study of cosmology and astronomy ; but he continues all his life to hesitate between the two systems of the world. He studies the *Almagest*, he borrows from Al Fargani what he says about the dimensions of the planets and of their orbits ; on the other hand he makes himself thoroughly acquainted with the theory of Al Bitrugi, the details of which hitherto had scared readers in France. In his *Opus tertium* he gives a carefully written summary of that theory, and then gives an account of the system of solid orbs described by Ptolemy in his *Hypotheses* and taken up by Ibn al Haitham. Bacon acknowledges that it does away with many of the objections formulated by Averroes against the epicyclic system, but he thinks that there are too many questionable hypotheses in it. His main objection is, that the two bodies between which each deferent is comprised have in their various parts different thickness,[1] but this cannot be the case in celestial bodies on account of their simple and homogeneous nature. He also thinks that a celestial body cannot be supposed to be devoid of all motion. Other objections made by Bacon seem to show that he cannot have been acquainted with the second book of Ptolemy's *Hypotheses*, the Greek original of which was probably lost before that time ; and that he only had before him an incomplete résumé of the systems of spheres

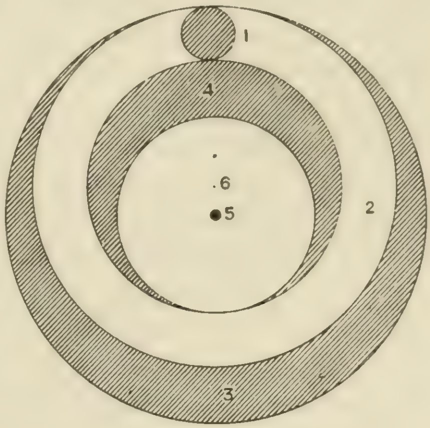



FIG. 2

1. Epicycle Sphere. 2. Excentric Sphere. 3. The Surrounding Sphere. 4. Complement of Surrounding Sphere. 5. The Earth. 6. Centre of Excentric Sphere.

[1] Compare the diagram in the *History of the Planetary Systems*, p. 259 (reproduced above), the surrounding sphere and its complement.

of Ptolemy and Ibn al Haitham.[1] Bacon therefore never reached any conclusion as to which system of the world was the true one.

But a younger contemporary of Bacon among the Franciscans, Bernard of Verdun, made up his mind to break with Aristotle and Al Bitrugi. In a *Tractatus optimus super totam Astrologiam*, of which two manuscripts are known to exist, and which shows familiarity with Bacon's works, he begins by counting up the facts which must be explained: the change of velocity of the planets; the variability of the moon's diameter, the moon being more or less completely eclipsed even in the same point of the ecliptic; the upper planets (particularly Mars) being brightest at opposition to the sun, while Mercury and Venus are of greater brightness after leaving the sun and moving eastward than when they leave it to move westward. All these facts distinctly contradict all homocentric theories, and Bernard therefore finally rejects the system of Al Bitrugi, with which he shows that he is well acquainted. In the system of spheres of Ptolemy or its imitation by Ibn al Haitham he sees the means of shielding the Ptolemaic system from Aristotelian attacks; and as the theory of excentrics and epicycles is the only one which is able to produce tables of planetary motions and 'save the phenomena', Bernard does not hesitate to proclaim it as the true system. The above-mentioned objection raised by Bacon he brushes aside by saying that the surrounding spheres, &c., are not proper celestial bodies, but like those parts of a cithern which do not give any sound.

This treatise by Bernard of Verdun seems, at least for the University of Paris, to mark the epoch when the Ptolemaic system began to reign absolutely among students of astronomy; the adherents of Al Betrugi had to give up the struggle. There are various other tracts in existence from the time around the year 1300 which confirm this result, while they show at the same time that the question about the precession of the equinoxes, whether it was a steady progressive motion or only an oscillation, was still unsettled, which involved uncertainty as to the number of spheres above those of the planets. The Alfonsine Tables, prepared under the direction of King Alfonso X of Castille, were finished about 1270, but they were probably not at once issued to

[1] The fact that he calls the system *Ymaginatio modernorum* also points to his only having known an Arabic account of it.

the public. At any rate it is certain that they were not known at Paris until towards the end of the thirteenth century, the tables of Al Zarkali (the tables of Toledo) being still in use there. The time had come however when some people among the Christian nations had begun in a small way to occupy themselves with practical astronomy, instead of merely speculating whether there were nine or ten heavens, or considering whether an astronomer or a philosopher was the best guide among the stars. There is a codex in the Bibliothèque Nationale which contains several tracts from the end of the thirteenth and the beginning of the fourteenth century bearing witness to this change of study. Among these are two by Guillaume de St. Cloud, one being an Almanac for twenty years from 1292, the other a Calendar, the date of which is 1296, which gives the time of entry of the sun into each of the twelve signs for two hundred years before and after the year 1296. At the beginning of the Almanac it is stated that the tables of Ptolemy of Alexandria, Tolosa (?), and Toledo do not agree with observations; there is no mention of the Alfonsine Tables. From observed solstitial altitudes of the sun in 1290 the obliquity of the ecliptic is found to be 23° 34′ and the latitude of Paris 48° 50′. Guillaume also determines the time of the spring equinox of 1290 (March 12, $16^h$) and corrects the errors of the Tables of Tolosa by the simple expedient of correcting the mean motions thus: of Saturn by $-1° 15'$, of Jupiter by $+1°$, of Mars by $-3°$.[1] M. Duhem does not explain what the Tables of Tolosa are, but a fragment of a tract by Guillaume de St. Cloud quoted by Nicolaus de Cusa gives the clue to this riddle.[2] The fragment gives the errors of the Alfonsine Tables for the three planets, and the amounts are exactly the same as those given in the codex as the errors of the Tables of Tolosa. This shows that the Alfonsine Tables had become known at Paris by the year 1292.

The same interesting codex also contains a tract by Johannes de Muris, a native of Normandy. He quotes the determination of the equinox of 1290 and describes how he repeated it at Evreux in 1318 (March 12, $16^h 40^m$); he says this agrees with King Alfonso's and Guillaume de St. Cloud's observations. Joh. de Muris and Firmin de Belleval wrote a report on the reform of the calendar, by order of the Pope, which if adopted would have settled this question more than two hundred years before the time when the

[1] Duhem, T. iv, p. 18.

[2] Ibid., p. 23.

Gregorian reform was actually carried out. Firmin wrote a little book on Meteorology which was printed at Venice in 1485.[1]

Another astronomer of the first half of the fourteenth century who must not be passed over here, though not connected with Paris, was a Jew, Levi ben Gerson of Avignon, who died in 1344. He wrote against the system of Al Betrugi and (what is more important) he invented (or at least introduced) the instrument known as Baculus Jacobi or Cross Staff for measuring the angular distance between two stars; and better still, he applied a diagonal scale to it. The latter invention (not mentioned by M. Duhem) appears to have remained unnoticed for about two hundred years.[2] Practical astronomy was also cultivated by Johannes de Lineriis (Jean de Linières), from whose hand there are several manuscript treatises in the Bibliothèque Nationale, among them a guide to the use of the Alfonsine Tables and *Theorica Planetarum*, anno Christi 1335. The latter is an account of the Ptolemaic System from the *Almagest* (without spheres); there is a chapter on the motion of the eighth and ninth spheres, in which it is shown that Tâbit's theory of oscillation must be rejected, as the equinox has now receded more than that theory allows; the author is inclined to adopt the theory proposed by Alfonso X combining progressive and oscillatory motion. Jean de Linières also produced a catalogue of positions of forty-seven stars, the first attempt in Europe to correct some of the star-places given in Ptolemy's Catalogue.[3]

[1] *Opusculum repertorii pronosticon in mutationes aeris.* M. Duhem (T. iv, p. 42) says that according to a manuscript copy in the Bibl. Nat. there was an interval of 68 years between the Alfonsine Tables and the epoch of certain tables; this gives the date of the book $1252 + 68 = 1320$. The sentence quoted occurs in the printed book on f. 12v at the foot, but the interval is 86, which gives 1338. The context shows that 86 is a misprint; yet compare f. 3v, where 1338 is given as the epoch of some star-places brought up from Ptolemy's catalogue.

[2] M. Duhem (T. iv, p. 40) says that the use of the baculus was introduced among Portuguese navigators by the German scientist Martin Behaim towards the end of the fifteenth century. But it has been conclusively shown by Joaquim Bensaude (*L'Astronomie nautique au Portugal à l'époque des grandes découvertes*, Berne, 1912) that the baculus was known in Portugal long before the time of Behaim, to whom and his compatriots the Portuguese owed nothing. M. Duhem quotes this book in a footnote without noticing that it demolishes what he has just stated in the text.

[3] See a paper by G. Bigourdan in the *Comptes rendus*, December 1915 and January 1916. The catalogue was printed in Riccioli's *Astronomia Reformata*, i, p. 216.

Other manuscripts from the fourteenth century show that various teachers at Paris continued to expound the Ptolemaic system, with or without spheres. At the beginning of the century Aegidius, a native of Rome, wrote a Hexaëmeron (printed several times in the sixteenth century) in which he proposes to let the epicycle-sphere lie, not between two spheres, but in a cavity in the celestial matter of the form of an anchor ring. Some of these writers had not a very clear idea of what they wrote about; e.g. Albert of Saxony in a Commentary to Aristotle's *De Caelo* (also printed several times) says that the moon does not move in an epicycle; for if it did, the figure of a man in the moon carrying a bundle of sticks would sometimes be seen upside down! He means of course that the moon would not always turn the same side to the earth.

Though M. Duhem several times alludes to the Alfonsine Tables, he has evidently not examined any codices of them. He has thus missed the interesting fact, that the tables first published at Toledo about the year 1270 were totally different from those first printed in 1483. The latter were a modification of the original tables made at Paris, probably by Jean de Linières. At Oxford, where there seems to have been a good deal of interest taken in astronomy from the beginning of the fourteenth century, the tables in their original form remained in use longer than anywhere else.[1]

While science had thus begun to be earnestly cultivated in France and England, Italy had remained behind. Though Gherardo of Cremona had translated the *Almagest* about the year 1175, more than a century passed before a single Italian studied it, while the text-book of Al Fargani was the usual guide. But it is remarkable how badly even that book was understood, the most extraordinary blunders being made by the best of the Italian writers. Dante is an honourable exception; there are no blunders in his cosmological ideas, neither in the *Divine Comedy* nor in the *Convivio*. The latter shows the craze for astrology which prevailed in Italy, while this branch of learning was at Paris always of secondary importance. The Italians took no part in the dispute whether a system of Physics deduced from peripatetic principles

[1] Cf. Dreyer, 'On the original form of the Alfonsine Tables', in *Monthly Notices of the Royal Astronomical Society*, vol. lxxx, pp. 243–62.

or a science constructed to agree with observed facts should conquer. Only Petrus de Abano (about 1300) alludes to it, but the dispute had been settled before he went to Paris. In the Universities of Spain and Portugal the fight did not begin till the middle of the fifteenth century and lasted about a hundred years, during which time objections were raised which had been demolished elsewhere long before.[1]

The second half of M. Duhem's fourth volume and the whole of the fifth deal with Arabian and Scholastic Philosophy. His account of mediaeval cosmology and astronomy reaches to the end of the fourteenth century, except that it does not deal with any English writers after Roger Bacon. The revival of astronomy in Europe has hitherto been supposed to date from the middle of the fifteenth century, and to have commenced with the labours of Cusa, Peurbach, and Regiomontanus in Germany. We know now from the researches of M. Duhem that the revival began in France fully a hundred years earlier, while the studies of M. Bensaude have shown that the scientific light spread by the Arabs in Spain and Portugal had never been put out, so that the navigators who found the way to the Indies and to the New World had nothing to learn from German astronomers. We shall look forward with great interest to the promised sixth volume of M. Duhem, while deeply regretting that his early death should have prevented him from completing this vast monument of learning and perseverance.

[1] In the south of France a last attempt to substitute spheres for epicycles and excentrics was in the fourteenth century made by Levi ben Gerson in his work *Milchamot Adonaï*, of which an account was published by Carlebach in 1910 (Duhem, v, pp. 201 sqq.).

# ROGER BACON AND THE STATE OF SCIENCE IN THE THIRTEENTH CENTURY

By Robert Steele

The real value of a man's work can only be estimated with any approach to accuracy when it is seen against the background of the intellectual life of his time: when his contribution to the world's thought is confronted with the ideas of his contemporaries, whether they run together in harmony or diverge into independent ways of development. The position of Roger Bacon in the history of science depends not only on the actual contributions to the stock of knowledge he made, but also, and more especially, on his mode of approach to the problems of his age, and on the foresight with which he selected the objects of his study.

The story of his life has often been told, and is tolerably familiar to most readers. Born about 1214, the year before Magna Carta, he first appears in history, if it be indeed the same, as a young clerk at the court of Henry III in 1233. Soon afterwards he left Oxford and went to Paris, at least for a time, before 1236. He seems to have travelled in Italy, dedicated a book to Pope Innocent IV, returned to Paris, lectured there as a Master in the University, and returned to Oxford about 1251. He joined the Franciscans about 1254, and towards 1257 was sent to their Paris convent, where he remained ten years. In 1266 he received the commands of Pope Clement IV to send him a fair copy of his works, but Clement's death in 1268 frustrated any hopes from him, and in 1278 he was condemned for teaching 'suspected novelties' and, tradition says, imprisoned till 1292, in which year he composed his last work, and shortly afterwards died.

The public life of Roger Bacon, then, extends from 1236 to 1272, the middle years of the great thirteenth century, almost the same as that of Louis IX, St. Louis of France, 1236–70. He was a contemporary of two great Popes, Gregory IX (1227–41) and Innocent IV (1242–54), and of the Emperor Frederic II, the new 'Stupor Mundi', who died in 1250. Two great Councils were held in his time at Lyons, and two Crusades were undertaken, while the Latin Empire of Constantinople came to its end and

the Greek rule there was restored. The Mongols had spread out and conquered from China to Silesia. Europe from end to end was filled with unrest and criticism of established authority, while authority was asserting itself with varying degrees of success. The strife between Pope and Emperor ended in the defeat of the Hohenstauffen and the extinction of his line, leaving the Imperial crown vacant for a score of years. Heresy stoutly fought a losing battle in the south of France, while in the north free cities established their rights under the royal protection. In England the controversies begun under John culminated in the summoning of a Parliamentary assembly by Simon de Montfort in 1265 before his defeat and death at Evesham. Among the masses this feeling of unrest manifested itself in opposition to the clergy, usually by a more or less good-humoured incredulity towards their personal character and teaching, but sometimes showing itself, as in the case of the revolt of the Pastoreaux, in the determination to destroy the clergy, suppress the monks, and turn on the knights and lords, as the Custos of the Franciscans of Paris puts it.

Civilized Europe was in those days comparatively small. Roughly speaking, from the Black Sea to the North Sea the northern boundary of civilization lay along the valleys of the Danube and the Rhine, and its common language was some sort of French, from England to Constantinople, and along the Mediterranean. French nobles ruled in Athens and Thebes, a French king gained the crown of the Sicilies, and everywhere there was a movement to and fro which made for internationalism. With the end of the thirteenth century this particular unifying influence had decreased almost to the vanishing-point, but others remained, only less potent. Every magnate of Europe, lay or clerical, had at some time or another of his career business to transact with the Papal Court: and this caused a continual stream of suitors journeying to Rome or returning from it to their own lands. Every prelate had to visit, at least once, the threshold of the Apostle, every ecclesiastical lawsuit found there its final court of appeal. The existence of the great Universities was another influence making for the international spirit. The readiest way to high promotion in the Church, and often in the State, for a poor man, was eminence in the law, Canon or Civil, and this eminence could only be attained in the schools of Bologna, while Theology, the Queen of Sciences, had her chosen seat at Paris,

and welcomed students from every country to her bosom. Lastly, modern commerce was beginning to appear in the great cities situated on the routes between the East and the West; and Florence, Genoa, and Venice were extending their relations in every direction within the ring-fence of Christendom, and sending outposts to the ends of the known earth. Interpenetrating all this was the Jewish community, whose solidarity and love of learning make the exact amount of its influence on Christendom hard to estimate.

The temperament of this great commonwealth is reflected in its popular literature, especially that of France. The two great vernacular works of Bacon's period are *Reynard the Fox* and the first part of the *Romance of the Rose*, the former a satiric criticism of life—of Church and State, not political but human; the second, gathering up all the poetry of its predecessors and veiling it in a symbolism instinct with study. No one can be said to have any real conception of mediaeval thought to whom these books do not appeal. The passionate love of beauty and the tolerance for human weaknesses of the age reveal themselves too in the architecture and the sculpture of such a church as Rheims was as much as in the literature of the day: they are a background which must be borne in mind while our attention is directed to other aspects of the thought of the day.

As the distinguishing feature of the late twelfth century was a revival of literature, so the thirteenth century was distinguished by a revival of science. The immediate cause of this was the introduction of the scientific works of Aristotle to western Europe, in nearly every case through the medium of Arabic translations. Up to the middle of the twelfth century the only works of Aristotle known were the *Categories* and *De Interpretatione* in the translation of Boethius, which, with the *Isagoge* of Porphyry and the *Timaeus* of Plato, represented to Western Europe the sum total of the scientific work of the Greeks. Not that Greek was unknown in these lands; on the contrary, a knowledge of it seems to have been fairly common, but the interest of the learned of the Byzantine Empire and of Magna Grecia lay in mysticism and devotional theology, while the ordinary speaker of Greek was about as fitted to translate Aristotle as an English merchant in an Asiatic port would be to translate modern philosophy or biology into the language of his surroundings.

It was in the Arabic-speaking countries that the new scientific movement began. Its great names are Avicenna (d. 1057), Algazel (d. 1111), and Averroes (d. 1198). Their commentaries on the text of Aristotle represented the highest point of the efforts of the battle between reason and faith, between the philosophers and the theologians of Islam. The battle spread from Islam to the Synagogue, but though the Mohammedan theologians finally crushed the school of Arabic Aristotelianism, its Jewish disciples carried on the tradition, and played a not unimportant part in its transfer to the Christian community.

The texts used by the Arabs were translations, in some cases made from Syriac versions, in others directly from the Greek; from manuscripts not always perfect, as may be seen from the phrase 'album in Greco'—a blank space in the Greek—so often met with in early Latin manuscripts of Aristotle. When these translations were turned into Latin, it was usually by means of two persons, a Jew or native who knew Arabic and gave the sense of the passage in Latin or the vernacular, and a writer who set down the passage in a more or less coherent and intelligent form. A good deal of our information as to these versions comes from the pen of Bacon himself, who knew personally some of the translators, and was markedly sensible of the faults in their work.

The spread of knowledge in the thirteenth century lay through three distinct channels, the Universities, the schools of religious houses both of the older orders and of the new friars, and through little groups and individual scholars working on their own account without any organization to back them up. The Universities were the chief means of spreading the study of philosophy and theology; the religious houses more or less preserved the chain of literary study and of historiography, in addition to their normal devotional writing; while the study of astronomy, cosmogony, alchemy, and such experimental science as existed was in the hands of small groups such as those of Pisa, Marseilles, and London, who issued astronomical tables for the calculation of the position of the sun, moon, and planets; of the alchemists of Italy, France, and, perhaps, England; and of such experimentors as Peter de Maricourt, the 'magister experimentorum', and others whose name and work are lost to us by their isolation and obscurity. Bacon himself for many years of his life seems to have belonged to this class.

It is, however, to the Universities that we must turn for the main influences on European thought, and more especially to Paris and Oxford. It was to them that the new Aristotelianism came, at first possibly as summaries of the text with the commentaries of Avicenna and Algazel, and, as curiosity was aroused, by more or less complete versions of the text of 'The Philosopher' himself (Aristotle) and of the works of 'The Commentator' (Averroes). The first of these versions that we can date with certainty were made in Spain about 1150 at Toledo, which long continued to be a centre of translation. It was from there that the new text-books came, according to Giraldus Cambrensis, 'libri quidam tanquam Aristotelis intitulati Toletani Hispanie finibus nuper inventi et translati, Logices quodam modo doctrinam profitentes', which led to the adoption of so many 'novitates et hereses' that their use was prohibited in Paris in 1210. These must have caused inquiry at Constantinople after its capture by the Latins in 1204, for in 1209 we learn 'legebantur Parisiis libelli quidam de Aristotele ut dicebantur compositi, qui docebant Metaphysicam, delati de novo a Constantinopoli et a Greco in Latinum translati, qui . . . iussi sunt omnes comburi', at the council next year.

Thus reaction triumphed for a while, and in the earliest statutes of the University of Paris (1215) dealing with the course of study we find that the only works of Aristotle studied were the old and new *Dialectics* and the *Nichomachean Ethics* (four books) with the *Topics* of Boethius. The prohibition was renewed by Gregory IX in 1231, but, long before, the *Physics* in one form or another were studied. Giraldus Cambrensis, in his old age (*c.* 1220), complains of young men arriving at the professorial chair (the M.A. degree) in three or four years instead of the twenty years of his own time, and devoting themselves to logic and physics instead of the poets and historians who used to be studied. William of Auvergne, who became Bishop of Paris in 1228 and was still writing in 1248, quotes the *Metaphysics*, the *De Anima*, the *Physics*, the *De Caelo et Mundo*, the *Meteorics*, the *De Animalibus*, the *De Somno et Vigiliis*, and the *Ethics*. Everything goes to prove that the end of the first quarter of the thirteenth century saw a nearly complete Aristotle in the hands of the Western world. But in practice these translations, passing through the hands of unnumbered and anxious copyists, soon grew to be wofully corrupt. Even Bacon

himself, lecturing at Paris, is led into accepting as a dictum of Aristotle the exact opposite of what now appears in the mediaeval texts, and we may perhaps attribute to this unfortunate experience a part of the criticisms which he lavishes on the ignorance of the translators.

It was not, however, the study of Aristotle that first aroused the mind of western Europe to the pursuit of science. Only one branch of study at that period could present in the fullest way the essential requisites for a complete science, the study of Astronomy. Its two branches, theoretical and experimental, allowed for close and detailed observation, the construction of theories founded on that observation, the prediction of future phenomena from theory, and the verification of these predictions, in a way that no other study could begin to approach : the only other scientific studies of that time, Medicine and Alchemy, being at the stage of empirical arts. Astronomy was another of the heritages of Europe from the Arabs. Since the time of the Greeks its greatest advances had been made in the country of its origin, at Bagdad. The *Almagest* of Ptolemy was translated into Arabic about A. D. 800 and reached Europe at the end of the twelfth century. The observatory at Bagdad was a centre of study, and here Thebit was led to his theory of the trepidation of the equinoxes to explain discrepancies between the calculated and observed positions of the heavenly bodies. From here a constant influence was exercised west and east from Granada and Cairo to Samarkand. With better observations, more correct tables were from time to time calculated and issued, and these tables were seized on by the Western world when they came into their hands, and adapted to their use. With each advance in precision came improvements in geometrical methods, more accuracy in the determination of periods like the length of the solar year or the relation of its length to the lunar month, to name no others. But in all this Arab science there was no fundamental difference from its Greek source, nothing was changed by the additions made, so that when it came into Europe it harmonized well with the traditional astronomy handed down from the Latins by Isidore of Seville and his copyists.

The science of Mathematics had grown with the needs of Astronomy : the art of the algorists had been improved by the publication of the *Liber Abaci* of Fibonacci in 1202, and was now

perfectly adequate not only for the ordinary needs of commerce but for the intricate calculations demanded by the observations of astronomers: a general method of extracting roots had been obtained, and was taught in the ordinary work of the schools. The works of Euclid had been translated by Adelard of Bath in the twelfth century and were revised by Campanus of Novara in 1246, and a sufficient amount of spherical geometry had been obtained for the needs of the day, while a sort of algebra and the elements of trigonometry only awaited a scientific nomenclature to be recognized as new branches of the science. What was needed to make it a vigorous study were better teachers and a wider outlook.

The science of medicine received a new impulse in the early part of the thirteenth century by the translation of the medical work of Avicenna, made by the order of Frederick II—a translation which dominated the teaching of medicine till the seventeenth century, and long survived its usefulness. It furnished a complete body of medical teaching, theoretical and practical, and, though Avicenna did not differ greatly from his Eastern predecessors, it introduced some new theoretical principles as a foundation or as an explanation of medical practice. But this translation was only one of the services rendered to medical science by the court of Sicily: a far greater one was the insistence on the value of dissection in the course of medical study. In 1231 the Emperor issued an ordinance forbidding any one to practise surgery, unless he had studied at least a year in human anatomy, and he is said (on, we are afraid, insufficient grounds) to have ordered the medical schools of Salerno to dissect publicly a human corpse at least once in five years. But it cannot be said that this practical side of the study of medicine has left much trace on the writings of the thirteenth century, perhaps because surgeons were of an inferior class to physicians, regarded as handicraftsmen simply obeying their orders, perhaps because the needful stimulus to original thought was drowned by the vast mass of literature thrust upon it.

Later in the century the University of Bologna was a great centre of medical teaching. An English bishop of Durham in 1241, Nicholas of Farnham, had been a professor of medicine in Bologna, but the formation of a scientific school there dates from about 1260 and is associated with the name of Thaddeus of Florence, who seems to have founded his teaching on Avicenna.

It is to Bologna later on at the end of the century that we owe the close connexion between medicine and astrology. Surgery was well taught there, judging by the many famous names of its pupils—Theodoric of Lucca, William of Saliceto, and Roland of Parma among others. Anatomy was taught by public annual dissections to which medical students of over two years' standing were admitted.

Of the Experimental Science of the early thirteenth century we know almost nothing except that it existed, and that some teachers of it were famous enough to be called 'Experimentator'. Thomas of Cantimpré quotes one such. Villars d'Honcourt, the thirteenth-century architect, has left us a sketch of a perpetual motion machine he had designed, the properties of the magnet had been studied, and it is difficult to believe that the fine architecture of the period was arrived at without an exact knowledge of mechanical law.

Chemistry and Alchemy were indistinguishable, but metallurgy was sufficiently advanced to ensure that the properties of the commoner metals and ores were known experimentally and that a theory to account for their more obvious properties should be evolved. One has only to consider the number of treatises quoted by Albertus Magnus to realize the mental activity expended on this subject. But no scientific study was possible until a much wider and deeper knowledge was attained, sufficient to indicate the points to which inquiry should be devoted. In the thirteenth century only empirical knowledge could be hoped for.

In this world of awakening science Roger Bacon began to take part somewhere about 1235, after having been at Oxford for some years, and having possibly attended the teaching of Grosseteste, who was lecturing in the Franciscan schools from about 1230 to 1235. There is no doubt that Grosseteste (*maximus naturalis et perspectivus quem vidi*) was the man from whom more than any other Bacon derived, not only much of his knowledge and many of his theories, but above all his love of mathematics and his insistence on the value of a mathematical demonstration. But certain features of his early teaching of astronomy in the notes of his lectures preserved by the Amiens manuscript lead us to the conclusion that Bacon's early studies were in the hands of more conservative teachers, whose astronomy was derived from that current before the invasion of Mohammedan science.

However that may be, Bacon seems to have come to Paris with a reputation good enough for him to be taken up by the Chancellor of the University, who advised him in the direction of further studies. Bacon was recommended to study medicine, and to apply himself to the composition of a work on some branch of it. The subject he selected was the relief of old age, and his *Epistola de accidentibus senectutis*, dedicated and sent to Innocent IV (1243–54), was the result. In the preparation of this work Bacon visited Italy ' in partibus Romanis ' and made himself acquainted with those works of Mohammedan medicine which had been introduced into Europe. His book is largely a compilation, perhaps one of the most complete on its subject that has been made, from Avicenna, Rhazes, Haly, Isaac, and Damascenus, and contains little that would mark out its writer as an independent or innovating spirit. It was after this visit that Bacon began the study of Greek, then a spoken and written language in southern Italy, as his treatise makes it pretty evident that he knew no Greek, Hebrew, or Arabic at the time, his knowledge of Greek medicine—Galen and Dioscorides—being obtained from Italy.

We owe to the piety or the industry, it would seem, of one of the pupils of Roger Bacon a report of several of the courses given by him as a regent master at Paris. They consist of two courses on the *Physics* of Aristotle and three on the *Metaphysics*, with another on the *De plantis*. An examination of the contents assures us that we have in the questions on the *Physics* the report of two distinct courses, not two reports of the same lectures, and the three sets of questions on the *Metaphysics* must belong to at least two courses and most probably three. As the *Physics* and *Metaphysics* were lectured on at the same time in 1245, and most likely earlier, we thus have records of three years' teaching—ending most probably in or before 1247. We put this date because Bacon, writing in 1267 or 1268, speaks of the twenty years in which he had laboured specially in the study of wisdom, after abandoning the usual methods.

These lectures bear the mark of youth—a youth of genius. They have been examined at some length by the late M. Duhem, who has made it plain that the principal subjects dealt with in them are those which preoccupied Bacon throughout his career, and that his treatment shows a progressive growth in knowledge and boldness of views. Even at this early stage of his career he

is an innovator, and it is to his exposition of the question as to the existence of a vacuum, that mediaeval science owed the theory which held sway for centuries till new facts were observed which showed its unsoundness, a theory crystallized into the expression 'Nature abhors a vacuum'. It is probable that the earlier course represents the views current at Oxford, the later was certainly given at Paris.

The style of these early works has little in common with that of twenty years later, and this offers a warning to us, forbidding the rejection of works attributed to Bacon merely on account of their style, provided that their subjects and treatment permit of their being placed among the 'multa in alio statu . . . propter iuvenum rudimenta' written by him, or the 'aliqua capitula nunc de una scientia nunc de alia . . . aliquando more transitorio'. There is, it is true, none of the fierce and outspoken criticism which marks his later work, but it must be remembered that few of the abuses he attacks had come into existence. His period of lecturing must have coincided with the last teaching of Alexander of Hales, who died in 1245, and the first appearance of Albertus Magnus in the Jacobin convent at Paris in the same year, and he had retired from his chair before St. Thomas came to lecture at Paris in 1252. His teaching compares favourably with that of Albert, as preserved in the Commentary on Aristotle's *Physics*, written most probably at or soon after this period.

It is in astronomy that these lectures show best the fluid state of Bacon's knowledge. In the earlier of them, that on the eleventh Metaphysics, his theory is a development of the Aristotelian reasoning in the *de Caelo et Mundo*. He seems entirely ignorant of the Ptolemaic theories, and to found his teaching on the Commentator of the *Timaeus* rather than on Alpetragius. His theories are those of the twelfth-century school of Chartres. In his second lectures on the *Physics* his knowledge of contemporary astronomical theory is greatly enlarged. He now finds the necessity of supposing nine orbs instead of eight, to account for the precession of the equinoxes, and though he is not yet prepared to acknowledge the opposition between the homocentric system of Aristotle and that of the school of Ptolemy, he is evidently on the road to this position.

We cannot with any certainty give a reason for Bacon's withdrawal from his work as a master at Paris. It may have been

due to the public affront he received, when, bungling over a strange word in the text of the *De plantis* he was explaining, and calling it Arabic, one of his students replied that it was Spanish. Certainly it is a curious fact that the reported lectures on this book break off just before the point at which this passage occurs. At any rate we may date from this period his resolution to become a master of the languages in which the fundamental documents of religion and science were written.

It was about this time that a revival of the teaching of mathematics in the University of Paris took place, following the republication of the Euclidean corpus by Campanus of Novara in 1246. Bacon tells us that he attended the lectures of many new masters on mathematics, but found that they did not understand the terminology of their subjects, so that he supplied them with geometrical proofs of positions from Aristotle and Averroes which none of the official critics in the disputations could attack. About this time (1248), Bacon seems to have learnt from Grosseteste his theory as to the origin of the tides and now probably began that intensive study of natural science of which his work bears such witness. He writes in 1267 that during the last twenty years he had spent more than 2,000 livres (a sum equal in purchasing power to something under £10,000 at present) in purchasing secret books, on various experiments, languages, and instruments, and astronomical tables, in forming friendships with the wise, and in instructing helpers in languages, figures, numbers, tables, instruments, and the like.

The experiments to which he devoted his attention may be gathered from his account of the mysterious Peter de Maricourt, whom he seems to have met about this time. The account of him in the *Opus Tertium* seems to be Bacon's ideal of a student. ' He makes no account of speeches and wordy conflicts but follows up the works of wisdom and remains there. He knows natural science by experiment, and medicaments and alchemy and all things in the heavens or beneath them, and he would be ashamed if any layman, or old woman or rustic, or soldier should know anything about the soil that he was ignorant of. Whence he is conversant with the casting of metals and the working of gold, silver, and other metals and all minerals ; he knows all about soldiering and arms and hunting ; he has examined agriculture and land surveying and farming ; he has further considered old

wives' magic and fortune-telling and the charms of them and of all magicians, and the tricks and illusions of jugglers. But as honour and rewards would hinder him from the greatness of his experimental work he scorns them.' We learn that in 1267 he had just completed a concave mirror capable of acting as a burning-glass, after three years' work, at an expense of 100 livres.

A reflection of this acquaintance may be found in the rather puzzling work known under the name of *De mirabili potestate artis et naturae*, consisting of ten or eleven chapters, of which the first six must have been written about this time, prompted, there is little doubt, by a recent denunciation by William of Auvergne, Bishop of Paris, of dealings in magic. The remaining chapters are of a different character and may possibly have formed part originally of another work.

The principal subject of these chapters is a delimitation of the wonders that can be wrought by an application of scientific principles from those which are mere trickery or those which may be due to the powers of evil. He seems to have gone thoroughly into the question of magic, and to have attended the thirteenth-century spiritualistic seances. 'When inanimate objects are quickly moved about in the darkness of morning or evening twilight, there is no truth therein but downright cheating and cozenage.' He allows a certain utility in charms, for example in the hands of a physician, that the patient may be induced to hope and confidence of a cure, quoting Avicenna as to the effect of the mind on the body. He then proceeds to give a general explanation of the way in which one person or thing can act on another, confusing what we now recognize as infection, sympathy, and hypnotism into one class of action at a distance, and while repudiating such books as pay worship to evil, warns his reader that many books reputed magical contain much useful knowledge.

He then proceeds to describe a few of these wonders, which give us some idea of what were really the mechanical problems of the time. The first is a ship, without men rowing yet moving faster on rivers or the sea than a galley. This seems to point either to an improved form of sails, or to something like a paddle moved with cranks. The car moving without animals to draw it may have depended on the same thing, the real obstacle to its use being the want of good roads. The flying machine with wings he acknowledges has only existed in plans. The small instrument

for lifting weights is evidently a sort of jack, unless it is a system of pulleys; similarly, the instrument by which one man can pull a thousand toward him. Walking under the sea, building suspension bridges, and so on, are also mentioned by him as possible at the time. He himself lays much stress on the properties of mirrors. Some of these appear obvious enough to us, such as the arrangement by which images of an object are repeated indefinitely, which it is difficult to believe had not been observed before, though he uses it to explain the existence of parhelia and mirages, and proposes its utilization in war. He also speaks of an arrangement of lenses by which distant things may be brought near and small things made visible: 'possunt enim sic figurari perspicue ut longissime appareant propinquissima, et e converso.' This is undoubtedly a description of the telescope without the enclosing tube. Such an arrangement is known to have been made by Leonard Digges, who died in 1571, from his study of Bacon's works. The magic-lantern is not obscurely hinted at, in a description of some optical illusions. But Bacon reserves his greatest praise for the use of lenses and mirrors as burning-glasses, and for the construction of a self-moving celestial globe, probably dependent on the use of a magnet, as was that of Peter de Maricourt. He further describes some inflammable mixtures and an explosive mixture which later on proves to be gunpowder.

We do not know how much of this period of investigation was spent at Paris, and everything points to the fact that Bacon returned to Oxford about 1251 and entered the Franciscan Order about 1253 to 1256. But during these years of absence from the University schools another and a rival influence was establishing itself there.

Albert the Great, the first doctor of the Dominican Order, had come to Paris in 1245; he was some years older than Bacon and brought with him a great renown as a teacher. He was accompanied by his pupil and socius Thomas Aquinas, now in his eighteenth year, whose fame was destined to put his own in the shade. Their task was to explain Aristotle and to reconcile him with dogma. In some respects this was difficult. Aristotle tells of no Creator, no Adam, no Providence, no eternity of the individual soul after death; and his commentators had to pass over these defects in silence, sometimes to deny the plain meaning of what he wrote, and occasionally to throw his teaching overboard altogether.

Albert's encyclopaedic work on the *Physics* and *Metaphysics* of Aristotle, begun perhaps before 1245 in view of his coming to Paris, was concluded by about 1256. His *Logic* and his *Summa* are parts of another scheme. It was written as a result of a demand from the Dominicans for a compendious account of physics which would enable them to understand the works of Aristotle on the subject. Albert felt himself unequal to the task, but after repeated entreaties set to work to write a Commentary for the use of the friars and of all those who desired to attain to natural science—a commentary many times longer than the original text. He goes on to describe the plan on which he worked, and to explain that there are three divisions of real philosophy; that is, philosophy not caused in ourselves by our own work like Moral Philosophy. These three parts are Natural Philosophy or Physics, Metaphysics, and Mathematics; and he promises to deal with each in turn. As a matter of fact he never did deal with Mathematics, and it is probable that he was not in a position to do so, but he dealt fully with everything included in the thirteenth century in Physics and Metaphysics.

Albert was, beyond doubt, one of the most learned men of his age, as Bacon, his severest critic, admits. Writing to the Pope in 1267 he mentions Albert and William de Shyrwood as the two most famous scholars of the day, and challenges comparison with them. In the *Opus Minus* he says of Albert, 'truly I praise him more than all the crowd of students, for he is very given to study and has seen an infinite number of things, and has spent much, and so has been able to collect many useful facts from an ocean of authors'. But Bacon insists that being a self-taught student he is not thoroughly grounded, seeing he was never trained by hearing, lecturing, and disputing in the ordinary arts course. Moreover, he does not know languages, and so cannot check the errors of his texts, and as he is ignorant of the laws of perspective he cannot fully explain any part of natural philosophy.

The dates of Albert's treatises are not well established, and the order in which they were written is only partly known, so that we cannot say how far Bacon's statement as to his ignorance of Greek and Arabic was true at the time. There seems no doubt that Albert consulted Greek texts and probably Arabic ones, though he may have had expert help. He certainly was a very keen and diligent observer; his tracts on mineralogy contain

many proofs of it. Perhaps this was because he could not find the complete book of Aristotle *de Mineralibus*: ' que diligenter quesivi per diversas mundi regiones . . . exul enim aliquando factus fui longe vadens ad loca metallica ut experiri possem naturas metallorum.' He goes on to describe his investigations among the alchemists, theoretical and practical, and compares his impressions with his own knowledge of how gold, silver, and other metals are found native or as ores. Of course these studies may have followed Bacon's criticisms and resulted from them. We know that Albert was sensitive to them.

But Bacon's criticism of Albertus, however strong, was, from the point of view that we share with him, well founded. The ideal science of Albertus Magnus was a science based on words and books, not on the phenomena of nature. He looked to logic for advance in knowledge. ' Et hoc est quod per investigationem rationis ex cognito devenitur ad cognitionem incogniti, hoc enim fit in omnia scientia quocunque modo dicta, sive sit demonstrativa ', he says in asserting the claims of logic to be a science. ' Investigatio enim sive ratio investigans ignotum per notum, speciale quoddam est.' It is true that he says elsewhere, ' Dicendum quod scientie demonstrative non omnes facte sunt, sed plures restant adhuc inveniende ', but these demonstrative sciences are further branches of logic, not sciences founded on observation of nature. Similarly the statement, ' Oportet experimentum non in uno modo sed secundum omnes circumstancias probare ', does not apply to experiments in natural science but to words. Summing up our impressions, his influence on his time is on the whole bad; his value for us is that he presents us with a *résumé*, from the scholastic point of view, of the knowledge already acquired by western Europe at the end of the first half of the thirteenth century.

Albert's great pupil, St. Thomas, enters late into the scheme of Bacon's surroundings. He may have been the ' Doctor Parisius ' on whom an attack is made, but his treatment of the Aristotelian system of nature seems to have been a little later in date than the *Opus Tertium*, and certainly to have avoided some of Bacon's criticisms by an attempt to obtain a new version made directly from the Greek. That this version was defective, too, was not his fault. Thomas was lecturing in Paris in 1252, after having spent three years there as a pupil of Albertus in 1245–8, and he

took his divinity degree at the same time as Bonaventura in 1257. His commentaries on the *Naturalia* of Aristotle are said to have been begun in 1263, and he lectured in Paris again from 1268 to 1272, when he left to take up a post in the reorganized University of Naples. The 'duo moderni gloriosi' of the *Communia Naturalium* must have been Albert and Thomas, but it is not till later that Thomas is mentioned (1271) in the *Compendium Studii*, with Albert as an example of the 'pueri duorum ordinum studentium', and it must be about this time or a little earlier (1268–9) that the strictures in the later part of the *Communia Naturalium* on his doctrines were written. 'Temporibus autem meis non fiebat mencio de istis erroribus.' But while these discussions have lost much of their meaning to us of this day, Thomas remains the greatest of the schoolmen, and his supremely intellectual attitude has carried over his theological work to our own days almost unaltered.

The work of Albert was not the only attempt to sum up the totality of human knowledge at the time. Several veritable encyclopaedias were composed about 1250, large and small, including all that was known or believed about the universe. Of these the most notable was that of Vincent of Beauvais, a Dominican like Albert and St. Thomas, who wrote between 1240 and 1264 his colossal *Imago Mundi* divided into three sections—the *Speculum Naturale*, *Speculum Doctrinale*, and *Speculum Historiale*, to which was later added by other hands the *Speculum Morale*. Perhaps Bacon had Vincent in mind when he wrote to Pope Clement that his own scheme could only be carried out by the aid of the Pope or of some great prince like the King of France, for it was to St. Louis that Vincent was indebted for the unrestricted use of the royal library, containing, it is said, some 1,200 manuscripts, and for the large staff of copyists and helpers necessary for the completion of the work. His method is to make extracts from the various authorities on each subject, and when necessary to add a few remarks of his own. It has been computed that he quotes from some 450 writers by name, some of whom we only know by his extracts.

The *Speculum Naturale* is divided into 32 books and 3,718 chapters, and is intended to be a full description of all created living beings, from angels down to fishes and plants. Book 15, for example, treats of astronomy and the calendar; books 23 to

27 deal with the psychology, physiology, and anatomy of man. In book 6, cap. 7, the question is discussed as to what would happen if a stone were dropped down a hole passing through the centre of the earth, and he decides that it would stay there. The *Speculum Doctrinale* treats of sciences and arts, beginning with the trivium, ethics, the economic arts, housekeeping, and agriculture, and so on. The eleventh book treats of the mechanical arts, which include architecture, navigation, alchemy, and metallurgy, weaving, smith-work, commerce, and hunting. Books 12 to 14 deal with medicine, theoretical and practical, and book 16 with mathematics. It is a most useful summary of the knowledge and belief of the time. The *Speculum Historiale* is, as its name implies, a gossipy abstract of the history of the world.

Another encyclopaedia, the most popular of all, was the *De Proprietatibus Rerum* of Bartholomew Anglicus, a Franciscan friar, in nineteen books. It is of the same general character as the *Speculum Doctrinale*, but shorter. It exists in hundreds of manuscripts and several early translations were made, while its popularity in the early days of printing was very great. It appears to have been written somewhere about 1250, for use, not in University circles, but among the general body of friars. The taste for encyclopaedic manuals spread to the laity, and we have a number of compositions in the vulgar tongue such as *L'image du monde* of Gautier de Metz, written about 1245, treating of cosmogony, astronomy, and geography, and the *Fontaine de toutes sciences*—a dialogue between King Boetus and the philosopher Sidrach on things in general—written a little earlier. The *Trésor* of Brunetto Latini, written in French about 1265, was a book of a superior sort, with as wide a range.

Bacon after these years of study, after repeated entreaties from his friends, and, it may be, criticisms from opponents, determined in his turn on the composition of an encyclopaedia which was to be a compendium of real knowledge, not a commentary on the works of Aristotle or on the compilations of other and lesser minds than his. The great work was to be founded on a reasoned scheme of mental progress, leading up to the supreme science of the mind, for him as for every one else in that century, the science of theology.

The scheme of his great encyclopaedia, as he conceived it, included four principal divisions: the first volume comprised

Grammar and Logic, the second Mathematics, the third Natural Science (Physics and Experimental Science), the fourth Metaphysics and Morals. He worked at this scheme until his task was arrested by his condemnation in 1277, only breaking off during the year devoted to the compilation of the *Opus Maius*, the *Opus Minus*, and the *Opus Tertium*, much of these works being obviously composed of material ready to hand. He seems to have worked at them in a desultory way, writing now on one subject now on another. 'Aliqua capitula de diversis materiis', 'nihil continuum est' are his words. This accounts for the great number and diversity of the smaller works that pass under his name, destined to take their place in a larger frame, and often unrelated to their fellows. Attempts have obviously been made since his death to effect this synthesis ; the fine Royal MSS. of Bacon have been through the preliminary stages of such an attempt. The Oxford MS. of the *Opus Maius* is evidently an aggregation of several tractates to the original work, and the Mazarine MS. of the *Communia Naturalium* offers undoubted proof of having been made up at the end of the fourteenth century from a number of independent fragments written for the *Opus Tertium*, and added to the work as it left the pen of Bacon.

The first volume of this encyclopaedia has not been handed down to us in any complete form. Several fragments of it remain : a nearly complete Greek grammar, a fragment of a Hebrew grammar, a *Summa grammatice*, and the tract published by Brewer under the title *Compendium Studii Philosophiae*. Most of them, in the form we have them at any rate, appear to have been written after 1267, though a part of what we find concerning Grammar and Logic in the *Opus Tertium* was probably taken from pre-existing fragments. Bacon returns to the subject in his latest work the *Compendium Studii Theologie*, written in 1292.

The second volume was intended to deal with Mathematics, pure and applied, of which nine divisions were recognized at the time, five speculative and four operative. This division is due to Alfarabi ; he divides geometry, arithmetic, astronomy (to use our modern nomenclature), and music into two sections each, theoretical and applied, and precedes them by a general study of principles 'de communibus mathematice'. We have a part of this treatise written before 1267, and a later and enlarged form of it written

some time after, together with some shorter additional tracts on Geometry. Astronomy was relegated to the third volume.

The third volume dealt with Natural Philosophy, the modern Physics. It was intended to include a treatise on the first principles of physics, the nature of rest and motion, of the four elements, of the theory of compounds both inanimate and animate, of generation and corruption, alteration, growth, and diminution. After this he proposed to treat of seven special sciences: Perspective, Astrology, the Science of Weights, Alchemy, Agriculture, Medicine, and Experimental Science. His scope is fully explained in the surviving parts of this volume, two forms of which exist, one written before 1267, the other after. We have in addition to those parts formally included in this volume a number of detached works, which would naturally fall into their place in the scheme, on Alchemy, Medicine, and Experimental Science.

The fourth volume of the *Compendium* dealt with Metaphysics and Morals. Of this we have a number of detached parts, a separate treatise on the lines of the *Metaphysics* of Avicenna, and a treatise on the propagation of force treated mathematically, which has been printed with the *Opus Maius*. There is no doubt but that he had completed a part of his treatment of the subject long before 1266, and two important treatises written at this period, showing the development of his theories, are still unprinted. A number of references to this work are to be found in the *Opus Maius*. The Morals would probably correspond largely to the seventh part of that work.

Not only had this great encyclopaedic compendium been undertaken, but Bacon had given considerable attention to a matter which was becoming rather urgent, the reform of the Calendar. The slightest knowledge of the beginnings of our history will remind us of the importance attached by early Christianity to the observance of Easter on the proper date. By the middle of the thirteenth century the Julian Calendar had amassed so great an error that it was perfectly possible that Easter might be celebrated a month later than the date on which, according to the spirit of the rule, it should fall.

Two works of Bacon on the subject are known. The large *de Computo* is an historical and general treatise on the various divisions of time and on the methods of using tables for the calculation of festivals or reducing Arabic to Christian dates.

That included in the *Opus Maius* is more controversial. It exposes the faults of the Julian Calendar, by the observance of which Easter is celebrated on the wrong date in the third and fourteenth years of the lunar cycle. He points out the superior advantage of using the Arab system of 30 years of 12 lunations, making 10,631 days, or of adopting the Hebrew system which is nearly the same. In this attack on the established order of things Bacon stands nearly alone in his time; and though his arguments were repeated in the next two centuries several times, it took three hundred years to effect this simple reform.

In connexion with his study of this work Bacon seems to have written several treatises, one *de Temporibus* which once existed in the Austin Friars' Library at York but is now lost; the long work, the *Computus*, written in 1263–4; and a shorter treatise dealing with times and seasons in 1266. He sums up his arguments in the fourth part of the *Opus Maius*, returning to the point in the *Opus Tertium*.

The turning-point in Bacon's career was in 1266. Probably about 1264, Cardinal Guy de Foulkes heard of Bacon's writings from a clerk in his service, Raymond of Laon, whom he commissioned to obtain them. In 1265 the Cardinal became Pope under the title of Clement IV. In March 1266 Sir William Bonecor was sent by Henry III on a special mission to the new Pope, and he seems to have carried with him a communication from Roger Bacon, which Professor Little conjectures, with some probability, to have been the *Metaphysica de viciis contractis in studio theologi*. On receiving this the Pope replied in a letter, first printed by Martene, asking him to indicate the remedies he would propose for the evils he had pointed out, and ordering him to send the writings he had previously asked for, secretly and without delay, any Constitution of his Order to the contrary notwithstanding. This letter is dated June 22, 1266.

As a result of this Papal command we possess the most widely known of Bacon's works: the *Opus Maius*—a preliminary work he calls it—a 'tractatus preambulus' in contradistinction to his great encyclopaedia, and with it three treatises, summaries and supplements to it: the introductory letter and summary found in the Vatican Library by Cardinal Gasquet; the *Opus Minus*—a summary with additional remarks on the causes of error in the church, Biblical textual criticism, and a short theory of alchemy;

and the *Opus Tertium*, a work of which the exact size is still uncertain, which was probably never completed. The *Opus Maius* and *Opus Minus*, with some other treatises, were probably sent to the Pope early in 1268, but as he died in the same year, and his successor was not elected till 1271, no answer was received by Bacon and no result followed from them.

Of the work of the succeeding years there is little to tell. Bacon seems to have worked on his *Compendium*, to have completed his introduction to the *Secretum Secretorum*, and about 1272 to have written the work published by Brewer. He was evidently in residence at Oxford at this period, to which may be referred the legends as to his construction of magical mirrors and perspective glasses referred to in the foundation for Greene's play of *Friar Bacon and Friar Bungay*. He was imprisoned in 1278 by the Minister-General of his Order for some ' suspected novelties ', and it is believed, on no definite grounds, that he remained in prison till 1292, the date of composition of his last fragment, the *Compendium Studii Theologie*, published by Rashdall—a work which was intended to be completed in seven parts, including much of his earlier writing.

It would be wrong to infer, because his name is rarely quoted in the few works of the half-century succeeding him, that his influence was negligible. Nearly all the manuscripts of his works we possess must have been written after his death. They range in date from the end of the thirteenth century to the middle of the fifteenth, and their form attests a constant attempt to edit and rearrange them according to the author's intention. In the face of his express condemnation, the wish that his ' dangerous teaching might be completely suppressed', the absence of his name is easily understood, but the influence of his thought is clearly discernible in Oxford teaching up to the time of the Renaissance.

In estimating Bacon's position among the men of his own time it is important to remember, first of all, the complete originality of his scheme. His great work, unfinished though it most probably was, and almost beyond the powers of any one unaided scholar to complete, the *Compendium Studii Philosophie* or *Theologie*, as the case may be, was as distinct in kind as in form from the works of his great contemporaries. As we have already said, it was an age of encyclopaedias, but none of them were independent in form. Albert's life-work consisted in a series of comments on

the words of Aristotle, following the order of his text with occasional excursuses when the subject seemed to suggest them, but breaking no new ground, and making no rearrangement of his matter. The great work of St. Thomas is conditioned by the *Sentences*, following its order with more originality and power than Albert, but, after all, adopting another man's scheme. His physics are mere commentaries on the text of Aristotle. On the other hand, the encyclopaedia of Vincent of Beauvais owes nothing of its arrangement to Aristotle, and comparatively little to its early forerunners, and was, so far, original. But there its originality ceased: it was merely a collection of facts and dicta collected from approved authors by the industry of a small college of clerks, and arranged under convenient headings. It added nothing to human knowledge, no inspiration for the progress of thought.

Bacon's schematic arrangement was not only unparalleled among the writers of his time; it was absolutely new. Nothing like it had been devised since the time of Aristotle, and it had the advantage of not being obliged to combat a large number of exploded hypotheses. The whole system of human thought was re-cast, and the plan simplified to an extraordinary degree to meet the necessities of the age. It may be that the framework of his scheme owed something to Al Farabi's *de Scienciis*, or to Avicenna, but in its conception and execution its originality is manifest. His plan has already been described, its execution was to be marked by the most rigorous economy. Everything superfluous or unnecessary to the development of the argument was to be cut away; all the excrescences which dialectical skill had embroidered on the simplest notions were to be discarded. Bacon did not exclude the notion of special treatises ancillary to the main lines of his course, but he did not regard them as necessary to every pupil. He thought it more necessary, for example, that the results of geometry should be known as a whole, than that the pupil should be able to prove the fifth proposition and be ignorant of the sixth book of Euclid.

The foundation of his system was laid on an accurate study of language: 'Notitia linguarum est prima porta sapientie.' Latin, of course, was taken for granted like English or French (is it, by the way, noticed that English is spoken by a Norman family as early as 1270 ?). But Latin alone was no sufficient key to the treasury of knowledge: its vocabulary was too limited,

the masterpieces of the world's teaching were either not translated into it, or were so badly translated that they were wholly misleading to such teachers and taught as attempted to profit by them. We need not labour the point as Bacon had to do; the fact remains that no mediaeval translation of Aristotle or his commentaries is now consulted, except as a curiosity. Bacon's study of languages, *Grammatica*, included not only the modern Philology but also a strictly utilitarian Logic and Dialectic, sufficient for the study of mediaeval science which was to follow: 'Grammatica et logica priores sunt in ordine doctrine.' We have a great part of Bacon's teaching on the subject, scattered over many writings. His main work was destructive; the schools were lumbered with inefficient text-books and antiquated errors. After that came reconstruction, 'secundum linguas diversas prout valent immo eciam necessarie sunt studio Latinorum'; Greek, Hebrew, and Arabic. He recognized three stages in the knowledge of languages, looking at the question from the point of view of actual life, not of a teacher. The first was that of a man who recognizes that a word is Greek, Hebrew, &c., can read it and even pronounce it, knows approximately what grammatical forms it may take, and so on: something like a man who can read enough German to know the names of the streets and the directions on the tramcars, but cannot speak the language. This is the sort of knowledge of Hebrew or Greek which Bacon pledged himself to impart in three days, when writing in his *Opus Tertium* and elsewhere. The second stage of knowledge was the power to read and understand in an ordinary way. 'Certes,' he says, 'this is difficult, but not so difficult as men believe.' His third stage is only reached when the student can talk and write and preach in the foreign language as in his own. The only grammars actually preserved to our own day which have been identified are the Greek and a fragment of the Hebrew. It is doubtful whether he ever knew Arabic or composed a grammar of that language. He certainly knew Chaldaic enough to explain its relation to and difference from Hebrew. His Greek Grammar is a very remarkable one for the time, even if it be true that it is founded on a Byzantine original. As we have it, the work is not complete, and the study of Greek made no progress in this country for nearly two centuries and a half.

This, then, is his first distinction in the study of languages,

that he laid down the principle that every language has an individuality of its own, and that the grammar appropriate for one of them, say Latin, would not lend itself to the study of another, such as Greek. It is a principle which was only recognized in practice towards the end of the nineteenth century, and is still disregarded by many writers. But he was also distinguished as a critic.

All human knowledge was, in his day, assumed to lead up to the study of theology—the queen of sciences. Whatever progress was made on the great lines of modern thought was made, not with that aim, but with the intention of facilitating the formation of right conclusions as to the relation between God and man. If we are to undervalue our predecessors on this account, we need not study the thought of the Middle Ages. Bacon's criticism of his contemporaries was actuated by the thought that they were bad teachers because they were insufficiently taught, that they taught in error because they were unable to test the truth of the maxims they repeated. His zeal for truth, his application of the tests of truth, remain of value if his conception of the highest truths no longer satisfies us. His criticism was applied to the text of the Vulgate ; the principles of textual criticism he laid down are of universal application.

A large part of Bacon's published work on the subject is taken up with the proofs of the corrupt state of the Vulgate text, made worse by the number of correctors, for the most part ignorant of both Greek and Hebrew. More than that, many of these corrections would not have been made if the correctors had even consulted a good Latin grammar. The scheme he proposes is an official attempt to restore the genuine text of the Vulgate as issued by St. Jerome from a comparison of the oldest manuscripts, which were to be collected, examined, and compared, while the readings were to be judged by the original Greek or Hebrew from which St. Jerome had made his translation. Here for the first time in the Middle Ages were the true principles of textual criticism laid down, principles valid for the work of every editor since his time.

After the science of language had been thoroughly mastered, so that the student was able to read the principal documents of scholarship in the original and follow their train of thought, Bacon next directed his attention to Mathematics. As we have

already pointed out, this science was already reviving in western Europe. In Italy Leonardo Fibonacci had published his *Liber Abaci*, Campanus had re-edited Euclid and written on the sphere, de Lunis had begun the study which was to become algebra. In France Alexandre de Villedieu had written the *Carmen de Algorismo*, the popular treatise on Arithmetic; in Germany Jordanus Nemorarius on the theory of numbers, the geometry of the sphere and on triangles; while the English John of Halifax wrote *c.* 1232 the *De sphera mundi*, the most popular text-book on the subject of the Middle Ages, and his *Tractatus de arte Numerandi*; and Peckham's *Perspectiva communis* and Grosseteste's semi-mathematical tractates were also published.

Bacon's own reading, as evidenced by quotations in his *Communia Mathematica*, was considerable. Besides the general scholastic learning of his day he quotes from all the works of Euclid, the *Almagest* and *Aspects* of Ptolemy, Theodosius on the Sphere, Apollonius, Archimedes, Vitruvius, and Hipparchus. Boethius is his main stand-by. The Arabic writers were well known to him, and early mediaeval writers such as Adelard of Bath, Jordanus, Anaricius, Bernelius, Gebert, and others are often quoted; indeed, we learn of a hitherto unknown work by Adelard from his writings. It would seem, however, that the general interest in mathematics of his time was strictly utilitarian. 'The philosophers of these days,' says he, 'when they are told that they ought to know perspective or geometry, or languages, and many other things, ask derisively, "What good are they?" asserting that they are useless. Nor will they listen to any account of their utility, and so they neglect and despise the sciences of which they are ignorant.'

His own view of the value of mathematics in education was a very high one. Like Plato, he saw in it the master key to all correct reasoning and all progress in knowledge. 'Mathematica est omnino necessaria et utilis aliis scientiis.' 'Impossibile est res huius mundi sciri, nisi sciatur mathematica.' 'Oportet ut fundamenta cognitionis in mathematica ponamus.' It was not so much mathematics for its own sake, as mathematics a handmaid to the natural sciences and theology. We have already referred to his classification of the subject as speculative and practical, following a study of the elements of the science. His remaining work is largely taken up by discussions of the meaning of con-

tinuity, infinity, dimensions, axioms, postulates, definitions, and the like, and he spends much time on the various ratios, arithmetical, geometrical, and harmonical. But in pure mathematics he was not an originator, he made no discoveries in geometry or the theory of numbers: his title to remembrance as a mathematician is his sympathy with it, his wide knowledge, and his insistence on its value as the foundation of a liberal education.

Bacon's work on optics was really a part of a larger scheme in his own mind—a study of the propagation of force at a distance. This he treated geometrically and used one variety of force as an example which was susceptible of measurement—light. Thus is explained the emphasis laid on the science of optics, or perspective, as he called it. His main treatises on it, the *Perspective* and the *Multiplication of Species*, were written before the *Opus Maius*. The scheme of the *Perspective* was not entirely new, of course; it had to include much that had been treated by his predecessor Alhazen, and, as a summary, to omit many of his detailed geometrical extensions of theorems. But on the other hand, it carried on the science a considerable way, as, for example, by proving that a concave spherical mirror would bring the reflected rays from different parts of its surface to a focus on different parts of its axis, and that to obtain a single focus the mirror must have a surface produced by the rotation of a parabola or hyperbola.

His study of the theory of optics went hand in hand with practical work; he caused to be constructed concave mirrors for use as burning-glasses, time after time, remarking on the diminishing cost as the craftsman grew more skilful; he was familiar with the use and properties of a convex lens both for magnification of objects, i.e. the simple microscope, and as a burning-glass; and there is every reason to suppose that he was acquainted with the combination of lenses which makes up the telescope, though he only used it for terrestrial objects and did not make it portable.

It is this combination which lies at the base of the legend of Bacon and Bungay's magic mirror which had grown up by 1385. 'Friar Roger Bacon took such delight in his experiments that instead of attending to his lectures and writings he made two mirrors in the University of Oxford: by one of them you could light a candle at any hour, day or night; in the other you could see what people were doing in the uttermost parts of the earth.

The result was that the students either spent their time in lighting candles at the first mirror instead of studying books, or, on looking into the second and seeing their relations or friends dying and lying ill, left Oxford to the ruination of the University—and so both mirrors were broken by the common counsel of the University.' This legend refers evidently to the burning-glass and telescope. The latter is shown to have existed by a statement printed in 1579 that Leonard Digges, then dead, ' was able by Perspective Glasses duely situate upon convenient Angles, in such sort to discover every particularitie of the Countrie round about, wheresoever the Sunne beames might pearse . . . which partly grew by the aid he had by one old written book of the same Bakon's experiments, that . . . came to his hands '. This work of Bacon's is no longer known to exist.

We have already spoken of his devotion to Astronomy in the modern sense of the word. His account of the science in the *Opus Maius*, the *De Celestibus*, and the fragment of the *Opus Tertium* published by Duhem, not only shows that he was abreast of the best work of his time, but also forms the best epitome of the state of knowledge at the day. His continual labour in the construction of astronomical tables bore fruit in his attempt at the reformation of the calendar, of which we have given some account. His work on Chronology, sacred and secular, is closely connected with this subject.

Bacon also takes rank as one of the earliest mediaeval writers on Geography, and part of his treatise was reprinted for the first time by Purchas in 1625 from the *Opus Maius* manuscript. His study was founded in the first place on Ptolemy, checked by modern travel, and his first consideration is an approximate determination of the relative amounts of land and water on the globe. It was a passage of this part of his work which had a leading part in deciding Columbus to make an attempt to reach the Indies by the Atlantic route, as shown by his letter from Haiti to Ferdinand and Isabella. Bacon's description covers the known world with the exception of western Europe, and he insists on the habitability of the earth south of the equator, and on the extension of Africa to the south. We have only to compare this treatise with Albert's *De natura locorum* to understand the great advance Bacon has made.

His position in the history of Chemistry has yet to be fully

investigated. The very large number of alchemical tracts which pass under his name show that his influence upon the students of the next century was very great. We are, naturally, at a loss to do more than form a reasonable conjecture as to the extent of his practical acquaintance with the operations of chemistry or alchemy, since his writings are devoted rather to theoretical than practical considerations. There was, of course, a large number of industries which depended on chemical reactions for their methods; brewing, dyeing, enamel-making, glass-making, metallurgy, lime-works, are but a few examples; but the eyes of inquirers were rarely turned towards these, and a superficial reason was given for the effects produced. What was really being sought by students was the general formula of the universe, adopting more modern terms, its integral equation, which, once found, would resolve any particular case by substituting suitable values for its constants. Whether he had made up his mind as to the existence of a universal primary matter in our modern sense, taking up the properties which made it a distinct material, is doubtful. The theories he held as to the action of the celestial bodies on this earth swayed him first one way, then another, while the dicta of Aristotle and Avicenna that no change can happen 'nisi fiat resolutio ad materiam primam', which he accepted without questioning, led him towards its acceptance. His doctrine of the multiplication of species was one of the most fruitful in his theory of alchemy. Just as celestial fire produced fire by means of a lens, so the celestial bodies might act on a suitable primary matter to produce their cognate metal, if their influence were as great. The science of weights was a branch of alchemy, because what distinguished the four elements in changeable matter from those in the super-celestial regions was their combination with the qualities of heaviness and lightness.

Alchemy, according to Bacon, was either speculative or practical. Speculative or theoretical alchemy treats of the generation of materials from their elements inanimate or animate. His list of inanimate things comprises metals, gems, stones, colours, salts, oils, bitumen, &c.; his animate things include vegetables, animals, and men. Alchemy was for him linked with Physics and Medicine in a chain of development. Among the treatises which give us the clearest views of his thought are the *Opus Minus* fragments and the *Opus Tertium*: by these the others

are to be tried and accepted or rejected. A striking example of the effect of his teaching is to be found in the treatise *De lapide philosophici*, attributed to St. Thomas Aquinas but really written by Fr. Thomas, chaplain to Robert, son of Charles of Anjou, in 1296.

Nothing has yet been said of his relation to the peculiar science of his scholastic contemporaries—speculative philosophy. This was almost a creation of his own time, due to the fuller study of Aristotle now possible. The meaning of matter, form, and substance, the struggle between realist and nominalist, the question of species and individual, all involved questions of the highest religious importance, pantheism or theism. Here Bacon, leading the Oxford schoolmen of later years, rejects much of the controversy as useless. There is no answer to the question what causes individuality or what universality: God makes things as their nature requires. The second part of the *Communia Naturalium* alone is quite enough to place Bacon in a high place among mediaeval schoolmen: its clear treatment shows solid thinking as well as sound criticism.

Looking back on the whole activity of this remarkable scholar we may try to sum up the interest he has for the modern world. Perhaps to himself the question would have been otiose: he was, like many men of science to-day, prepared to accept results and methods without lingering over the history of how they came into being. But on the other hand, to the large and increasing number to whom the history of scientific thought and method is often almost as important as its results, Roger Bacon stands out prominently as the first English leader of scientific thought. More still, he has the special English quality of fighting a lone battle for his views, unsupported as he was by his own order, attacking its opponent's chiefs, and remaining unshaken to the end. His works, though not entirely neglected, have usually been treated as curiosities, while those of his two great contemporaries have been held in reverence for centuries, and even to-day are receiving the full honours of scholarship in new editions from the manuscripts. The publication of his remains would be invaluable, if only as marking the development of a mediaeval thinker, ranging as they do over a period of forty years' activity from his early lectures at Paris. In them we can trace the process of emancipation from established ruts of thought and the entrance of new conceptions.

We can follow his attempts to make a theory to explain the whole body of natural phenomena, the gradual elaboration of a mathematical theory of action at a distance, which, unfruitful at the moment, reappears in a fuller form in modern science. We see him as a pioneer of textual criticism, a critic of established authorities in whom the spirit of *Reynard the Fox* and the *Fablaux* is incarnated, a critic of received doctrines who applies to them in an ever-increasing degree the test of common sense and experiment. The work of such a one should be available to all the world of scholars : more than half of it in bulk is still locked up in single manuscripts difficultly legible and almost inaccessible.

Leonardo da Vinci (1452–1519)

From a crayon portrait by himself at the Royal Library, Turin

Quaderni V fo. 8 r

*Foetus in utero* and relations of membranes to uterine wall

# LEONARDO AS ANATOMIST

BY H. HOPSTOCK

TRANSLATED FROM THE NORWEGIAN BY E. A. FLEMING

Wisdom is the daughter of Experience,
Truth is only the daughter of Time.
LEONARDO.

THE greater part of Leonardo da Vinci's anatomico-physiological manuscripts are preserved at Windsor. Sixty leaves of these, with altogether about 400 drawings, have been published in facsimile with a diplomatic transcription and French translation as *Fogli A*, Paris, 1898, and *Fogli B*, Turin, 1901, the two together constituting the edition of the Russian Sabachnikoff and the Italian Piumati, with a preface by the French anatomist Duval. The remaining Windsor manuscripts of 129 leaves, with altogether about 1,050 drawings, have been published in facsimile with diplomatic transcription and English and German translations as the six volumes of the *Quaderni d'Anatomia* by Vangensten, Fonahn, and Hopstock, Christiania, 1911–16.

The facsimiles in the *Fogli* show Leonardo's drawings without colour. The *Quaderni* contain an exact reproduction of his manuscripts, showing the various tones of the paper, and the shades of the red chalk, pencil, ink, and other pigments which he used. The drawings and text of the Fogli, with very few exceptions, treat of anatomical and physiological questions only. Of the Quaderni, three-quarters are concerned with these subjects, whilst a quarter of the drawings, and much of the text, deals with other matters, especially mathematics, geometry, physics, and art.

The Quaderni show more clearly than the Fogli the manner in which Leonardo carried out his anatomical researches, and the period during which these developed. The pages of the Fogli, on the whole, reveal Leonardo as an independent and confident anatomist, especially with regard to his drawings. These manuscripts must therefore have been written in his later years. They discuss osteology, myology, the peripheral nervous system, the

blood-vessels, the abdominal organs, and so on, all in a comparatively fluent and clear style. The Quaderni, on the other hand, cover a very long period in Leonardo's investigations, from his very first anatomical studies in 1489 up to his latest years. His language in the Quaderni is consequently not infrequently uncouth and involved, so that it is difficult to make out the meaning. The style, however, is extremely characteristic throughout.

Leonardo apparently at first made extensive use of old anatomical literature and diagrams. He attempted to elucidate and explain these, but as his authorities were inaccurate, so also were his delineations indefinite and clumsy. He then began to pursue his own studies, cautiously and tentatively at first, and then with more ease and definite purpose, until at last, emancipated from tradition, he stands forth as what he is, a great biologist.

He carried out his own precept to dissect the same part repeatedly. This is evident from the Quaderni, where sometimes on the same page, sometimes on different folios, numerous sketches appear of the same organ, sketches which show his progress from a hesitating student to a confident and independent investigator.

The Quaderni show also that Leonardo dissected animals. He has in part applied these discoveries to man, and it is evident from some embryological drawings and sketches of the processes of reproduction that there are instances in which he rested satisfied with these, though for the most part he finally portrays conditions as they occur in man. Yet strange to say, in the midst of his best topographical work, he occasionally takes an illustration of a single organ from an animal ; it would seem that he has done this but as a trial or experiment, for in other cases the organ is correctly reproduced.

Certain departments of anatomy, only lightly touched on in the Fogli, are made the subject of careful study in the Quaderni. Such, for example, are the study of embryology, the structure of the generative organs, the form and functions of the diaphragm, the lungs, the brain cavities, and especially the heart and vascular system, as well as surface anatomy, and the study of proportion. It is indeed evident from the Quaderni that Leonardo undertook his dissections, not only in order to obtain anatomical data, but also, with the aid of this knowledge, to arrive at a clear understanding of physiological processes. He therefore makes his

anatomical and physiological researches conjointly, expending not infrequently more work on the latter.

Great physicist and mathematician as Leonardo is, he corroborates his physiological investigations by experiments and by proofs and tests drawn from physics and mathematics ; several of his dicta emphasize this fact : ‘ He who is not a mathematician according to my principles ’, he says, ‘ must not read me ’ ;[1] and again, ‘ Oh, students, study mathematics, and do not build without a foundation ’.[2] For Leonardo the naturalist it was a matter of course to seek the principles of movement in animals in the laws of mechanics.

Under the heading *On Machines* he indicates four primary natural forces : (1) local movement which is produced by the three other forms of movement, (2) natural weight, (3) force and (4) *percussion.*

‘ We shall therefore ’, he says, ‘ first describe this local motion and how it produces and is produced by each of the three other Powers. Then we shall describe the natural weight, although no weight can be termed otherwise than accidental ; but so it has pleased (us) to call it, to distinguish it from the force which is, in all its operations, of the nature of weight and is therefore called accidental weight ; and this is set up as the third Power of Nature or the one produced by Nature. The fourth and last Power shall be called percussion, i. e. end or impediment of motion. And we shall first mention that every local involuntary motion is produced by the voluntary motor, like the counterpoise of a clock lifted up by its motor, Man.’ [3]

Thus in his opinion every local or involuntary movement is, in the ultimate analysis, produced by a motive power that is itself voluntary, just as the involuntary movements of a clock depend ultimately on the voluntary movements of him who raises the clock weight.

‘ Furthermore, the Elements mutually repel or attract each other, as one sees that water expels the air and the fire entered as heat into the bottom of the cauldrons and escapes through the bubbles on the surface of the boiling water. And again the flame attracts the air, and the heat of the Sun draws up the water in the form of moist vapour, which afterwards falls down as heavy rain ; but percussion is the immense Power of things which is produced in the Elements.’ [4]

[1] Q. iv, f. 14 v. [2] Q. i, f. 7 r. [3] Q. i, f. 1 r. [4] Q. i, f. 7 r.

Leonardo's general idea of the proper method for the investigation of the human body and its parts is given in his *Plan for the Book*:

'This my exposition of the human form', he says, 'shall be demonstrated to you not otherwise than if you had the real man before you; and the reason is, that if you want to know thoroughly the parts of a dissected person, you must turn him, or your eye, examining him from different aspects, from below, from above, and from the sides, turning him, and investigating the origin of each member, and in this way the natural dissection has satisfied you as to your knowledge. But you must know that such knowledge does not satisfy you on account of the great confusion of pannicles (membranes) with veins, arteries, nerves, tendons, muscles, bones, and blood, which colours each part with the same colour, and the vessels which empty themselves of blood are not recognized on account of their diminution; and the integrity of the membranes is broken by the examination of the parts which are enclosed in them, and their transparency, tinged by blood, prevents you from recognizing the parts covered by them on account of the similarity of their blood-colour; and you cannot learn to know these parts without confounding and destroying the others. It is therefore necessary to do several dissections. . . . Thus each part and each whole will become known to you by my diagrams with the aid of demonstrations from three different aspects of each part. . . . Accordingly, the cosmography of the microcosm (*minor mondo*, i. e. man) will be demonstrated to you here through 15 full figures in the same order as has already been adopted before me by Ptolemy in his cosmography of the macrocosm (i. e. the world).'[1] And Leonardo adds: 'You must in your anatomy, depict each phase of the parts from man's conception until his death and till the death of his bones, stating which part of them decays first, and which part of them lasts longer.'[2]

Leonardo was a well-read man, conversant with the anatomical writings of Galen, Avicenna, Mondino, and Benedetti, but his opinion of authors is apparent from many passages in his works.

'I do not understand', he says, 'how to quote as they do from learned authorities, but it is a much greater and more estimable matter to rely on experience, their masters' master. These men go about puffed up, and boasting, adorned, not with their own qualifications, but with those of others, though they will not admit mine. They scorn me who am a discoverer; yet how much more do they deserve censure who have never found out

[1] Q. i, f. 2 r. [2] Q. vi, f. 22 r.

anything but only recite and blazon forth other people's works. . . . Those who only study old authors and not the works of nature are stepsons, not sons of Nature, who is mother of all good authors.'[1]

The depth of feeling which animates Leonardo during his work of dissection can be gauged from the following passage :

'O searcher of this our machine, you must not regret that you impart knowledge through the death of a fellow creature ; but rejoice that our Creator has bound the understanding to so perfect an instrument.'[2]

And what demands he makes on the dissector are seen in the following :

'And if you have love for such things you may be prevented by nausea ; and if this does not hinder you, you may be prevented by fear of living during the night hours in the company of these quartered and flayed corpses, hideous to look at ; and if this does not deter you, perhaps you lack the good art of draughtsmanship, which is essential for such demonstrations, and if you have the art of drawing, it may not be accompanied by the sense of perspective, and even if it is, you may lack the order of geometrical demonstrations, and the method for calculating the force and strength of the muscles ; or perhaps you lack patience, so that you will not be painstaking. . . .

'As to whether all these things have been in me or no, the hundred and twenty books written by me will furnish sentence, yes or no, for in these I have not been hampered by avarice, or by negligence, but only by time. Vale.'[3]

Leonardo's nomenclature is very deficient. Bones, muscles, nerves, and vessels, have, as a rule, no definite names but are indicated by letters or some such means. In the case of the bones, the old names that were in use by mediaeval writers often occur, for example, *adiutorium* for humerus, *furcula* for clavicle, *focile maius* and *minus* for the ulna and radius ; the muscles are also indicated by their origins and insertions, thus *pars domestica* and *pars silvestris* describe the palmar and dorsal sides of the extremities, *rascetta* and *pecten manus* indicate carpus and metacarpus ; and then there are the mediaeval Arabic terms *meri* for oesophagus, *sifac* for peritoneum, and *mirac* for abdomen.

Leonardo's representation of embryological conditions is naturally by no means complete. He seems to have examined the

[1] Quoted from Oswald Sirén's *Leonardo da Vinci*, Stockholm, 1911.
[2] Q. ii, f. 5 v.
[3] Q. i, f. 13 v.

embryos of animals, hens, and calves, before he studied the human foetus. He says in fact: 'But you must first dissect the hatched egg before one shows the difference between the liver in the foetus, and the fully developed human.'[1] One of his figures[2] is evidently taken from a bird's egg, and he counsels one to observe 'how the bird nourishes itself in the egg'.[3] He remarks that chickens can be hatched by the warmth of an oven.[4]

'Ask the wife of Biagin Crivelli how the capon rears and hatches the eggs of a hen, when he is intoxicated. Her chickens are given into the care of a capon, which is plucked on the underside and then rubbed with nettles and set under a basket; then the chickens go in under it, and it feels it is being tickled by the warmth and likes it, for which reason it afterwards leads them and fights for them, jumping into the air against the goshawk in ferocious defence.'[5]

He seems also to have sought after the cause of difference of sex, and considers he has found it in that 'eggs that are round-shaped produce males, and those that are long-shaped produce females'.[6]

Leonardo has a series of very beautiful drawings of the human foetus lying in the uterus[7] (Plate XXVI). The position of the foetus is correct, and apparently he must have had opportunity to dissect a gravid uterus, but much of his work must have been done on the foetal calf. The foetus is surrounded by three membranes, the 'animus', 'alantoydea', and 'secondina', probably corresponding to the amnion, allantois, and chorion of our notation. He does not describe a placenta, but on the other hand his drawings show how the chorion connected in several places with the inner surface of the uterus by cotyledons, the *male cotyledons* on the chorion embracing the *female cotyledons* on the uterus.

'The child in the uterus', he says, 'has three panniculi which surround it, of which the first is called *Animus*, the second *Alantoydea*, the third *Secondina*; with this Secondina (chorion) the uterus is conjoined by means of the cotyledons, and all join in the umbilical cord, which is composed of vessels.[8] . . . How the three panniculi of the uterus bind themselves together by means of the cotyledons . . . female and male cotyledons. . . . Let some one give you the secondina of a calf when it is born, and observe the form of the cotyledons whether they retain the male or female

[1] Q. i, f. 10 r. [2] Q. iii, f. 8 v, fig. 3. [3] Q. iii, f. 9 v. [4] Q. iii, f. 7 r.
[5] Ibid. [6] Ibid. [7] Q. iii, ff. 7 r. and 8 r. [8] Q. iii, f. 8 v.

cotyledons; I observe how the fetal membranes are joined to the uterus, and how they loosen themselves from it.' [1]

The word *secondina* is thus used by him in various senses: as chorion, as secundine, and as foetal membrane generally.

The foetus does not breathe in the uterus, for it would drown if it did so, lying, as it does, in water.[2] Leonardo considers as the reason for this arrangement that heavy things weigh less in water than in air,[3] and that the foetus does not require to breathe because it is animated and fed by the mother's life and nourishment.[4] He denies the truth of the old story that the foetus cries or wails in the uterus, and considers that any sounds that seem to emanate from a gravid uterus must be caused by maternal flatus.[5] He frequently remarks that 'one soul governs (the) two bodies', and that what the mother eats, and the impressions she receives, leave their mark on the foetus.[6] He defines the length of the full-grown foetus as a *braccio* and the length of an adult as three times that of the full-grown foetus.[7] He has examined a foetus which was less than half a *braccio* in length at nearly four months.[8] He remarks 'how in four months the child is half of its length, i. e. eight times less in weight than when it is born.' [9] He draws attention to the fact that the foetus in the uterus grows three times as quickly as the new-born infant, and that a year after birth a child has not yet attained to twice the length of a nine-months-old foetus.[10] He has examined the viscera of the foetus, and draws attention to the relatively greater size of the left lobe of the liver at this stage of development, and points out that it diminishes after birth.[11] He seems to have observed the conversion of the umbilical vein of the foetus into the round ligament of the liver of the child.[12]

As a result of his study of conception and of the growth and birth of the foetus, Leonardo comes to the conclusion that man and his works are in no way things isolated in nature, but are only a part of one great whole, a single link in one vast chain, so that man is subject to the same 'necessità' as all other living things. He formulates these thoughts in the following words: 'Every seed has an umbilical cord which breaks when the seed is

[1] Q. iii, f. 8 r. [2] Q. iii, f. 7 r. [3] Q. iii, f. 1 v. [4] Q. iii, f. 8 v.
[5] Q. iii, f. 7 v. [6] Q. iii, ff. 3 v. and 8 v. [7] Q. iii, f. 7 r.
[8] Q. iii, f. 7 v. [9] Q. i, f. 10 r. [10] Q. iii, f. 7 v.
[11] Q. iii, f. 8 v and Q. i, f. 10 r. [12] Q. iii, f. 10 v.

mature. And similarly they have matrix and secondina as the herbs and all the seeds which grow in pods show.' [1] In this connexion we meet one of Leonardo's characteristically abrupt transitions of thought. While sketching a foetus in the intra-uterine position with elbow bent and hand prone, he suddenly asks himself which muscles flex the elbow-joint, and, immediately under the drawing of the foetus, makes two rough sketches of the muscles of the arm with pronated hand and the following legend attached. 'Demonstrate here only those muscles which serve to bend the arm to a right angle and those which cause it to turn the hand back and forward. Do not concern yourself with anything else, but demonstrate only the functions performed by those muscles which rise immediately from the bone of the said humerus.' [2]

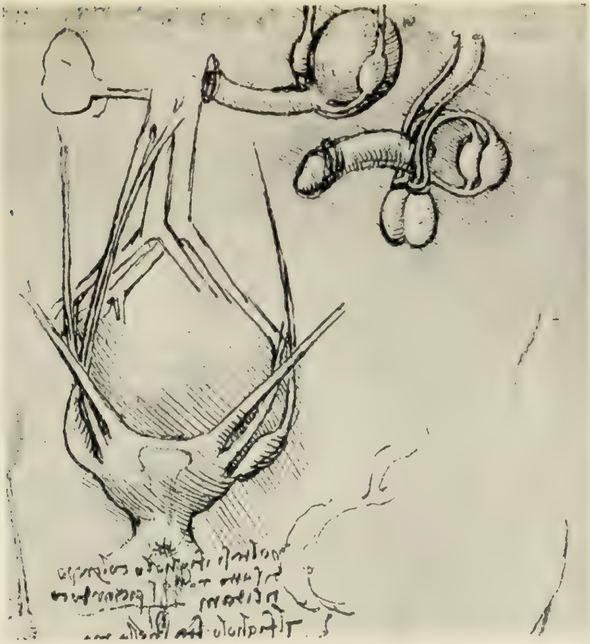
Leonardo differs from many of the mediaeval writers who preceded him in representing the uterus with only one cavity. The tubes go outwards and upwards and the ovaries lie to the side of the uterus. On the right the artery of the ovary is seen to come from the aorta and the vein to go to the vena cava inferior (Plate XXVII). The ovaries are designated as 'seed-vessels (*vasi spermatici*) in the form of testicles, and her seed is first blood like that of the male '.[3] He is aware too that there is a difference between the male and the female pelvis. 'Measure how much less the woman's pubic bone is than the man's. It is for the sake of the space between the lowest part of the pubic bone and the point of the coccyx in view of parturition.' [4]

It is apparent from various drawings that Leonardo had a fairly good knowledge of the structure and relations of the testis, vas deferens, vesiculae seminales, vasa spermatica and nervi spermatici interni (Plate XXVII), 'nerves, originating in the vertebral column, which join the vein of the testicle ',[5] although here also he partly transfers his findings from animals to man.

Leonardo discusses the results of conception from the union of white and black parents, and remarks that the colour of the offspring is not conditional upon the influence of the sun but of the parents' colour, and states 'that the mother's seed has an influence on the embryo equal to that of the father '.[6]

A number of general observations on Osteology are found in

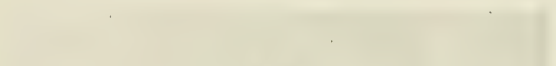
[1] Q. iii, f. 9 v. [2] Q. iii, f. 7 v. [3] Q. iii, f. 1 v.
[4] Q. iii, f. 4 v. [5] Q. iii, f. 3 r. [6] Q. iii, f. 8 v.



Quaderni III fo. 1 v

(Left) General structure of uterus and sources of its blood supply.

(Right) Male organs.



Quaderni V fo. 18 r

Topographical anatomy of neck and shoulder in a thin aged individual.

the Quaderni, which could only have been written by one well versed in the subject. In the *Plan for the Book* occurs this passage :

'It is necessary to make three dissections for the anatomy of the bones, which must be sawn through to demonstrate which is perforated and which is not, which is medullary and which is spongy, and which from without inwards is thick and which is thin, and which at one place has great thinness, and at one place is thick and at one is perforated or full of bone, or medullary or spongy, and thus all these things will sometimes be found in the same bone, and there may be a bone which has none of them.' [1]

In another place, Leonardo writes: 'Bone is of inflexible hardness adapted for resistance, and is without feeling. It terminates in cartilages at its extremities. And the medulla is composed of sponge, blood and soft fat covered with the finest veil. Spongy bone is a substance composed of bone, fat and blood.' [2]

Leonardo's treatment of the hands in his paintings is well known. Under the heading *The Hand from the Inside* he demands that the bones shall first be studied in order, so that their number, shape, and position should be learnt, and afterwards they should be further examined by being sawn through ; they must then be put together according to the articulations ; then the muscles that connect carpus with metacarpus and the tendons which move the first, second, and third joints must be demonstrated ; next the nerves, arteries and veins, then the hand as a whole with the skin must be examined, and lastly the measurement of the hand and its parts must be given. A similar systematic procedure must be adopted in dealing with the back of the hand.[3]

With the exception of two delicate sketches in red chalk of the bones of the lower limbs set at the correct inclination to the pelvis (Plate XXXV), the Quaderni—in contrast with the Fogli—contain no osteological drawings of great interest, but only a few rough sketches, mostly of the bones of the extremities. A drawing of the cranium, the cervical, and part of the thoracic vertebrae has no close relation to the actual facts.[4] This drawing must date from the earliest period of Leonardo's anatomical studies, before he had begun to dissect, and when his fantasy had free reign, working rather on information gained from books, and possibly from old drawings that have now disappeared than on actual observation.

[1] Q. i, f. 2 r. [2] Q. ii, f. 18 v. [3] Q. i, f. 2 r. [4] Q. ii, f. 5 v.

As regards the morphology of the muscles, Leonardo writes :

'Muscles are of many kinds, some without tendons, like the trabeculae in the right ventricle of the heart, and others similar. Some are round like the above-mentioned and isolated (*musculi papillares*), being connected only by tendons (*chordae tendineae*) with that of the flexible part. . . . Some are broad and thin, some broad and thick, some long and narrow, others long and thick ; some are thin and oval, some shaped like a fish, others like a lizard, some are twisted and some straight. Some have tendons along one side only, others at both ends, others are divided by several tendons, as for instance the longitudinal muscles. Some may move the part from either end, others from one end only, another moves behind its tendon, others draw their tendons towards themselves.' [1]

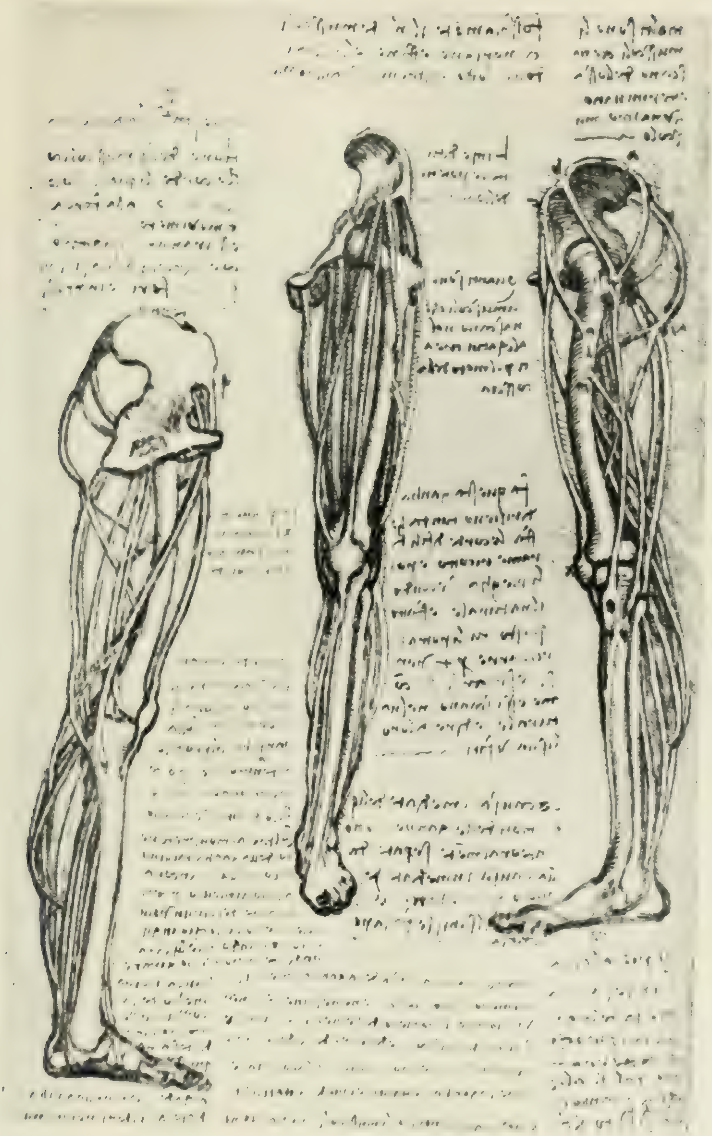
Leonardo states that muscles move longitudinally,[2] and he speaks generally of muscles with several heads.[3] In a passage on *Definition of the Instruments* he writes : 'The muscles, the functionaries of the nerves, draw to themselves the tendons which are connected to these members. . . . The tendons are mechanical instruments which in themselves have no feeling, but carry out whatever is imposed upon them.' [4]

In order to demonstrate the structure of the lower limb and the actions of its muscles he gives three illustrations of models formed from copper wire (Plate XXVIII). These are in the main correct and to them Leonardo attaches the following text :

'How many muscles originating in the hip are formed for the movement of the femur ? Present the leg in full relief and make the cords of red-hot copper wire, and bend them on it to their natural position, and when you have done this you will be able to sketch them from four sides, and place them as they are in nature, and discuss their functions. When you have finished with the bones of the lower limb, give the number of all the bones, and having completed the tendons, give the number of these tendons, and you must do the same with the muscles and the nerves, the veins and the arteries, stating : the thigh has so many, the leg so many, the foot so many, and the toes so many, and then you must say : so many muscles spring from a bone and end in a bone, and there are many that spring from a bone and end in another muscle, and in this manner you can describe each detail of every part of the body, and especially the ramifications made by some muscles which produce various tendons.' [5]

[1] Q. ii, f. 15 r. [2] Q. iv, f. 6 r. [3] Q. iii, f. 9 v.
[4] Q. ii, f. 18 v. [5] Q. v, f. 4 r.

PLATE XXVIII



Quaderni V fo. 4 r

Bones of lower limb to which wires are fitted to illustrate lines of muscular traction

Leonardo emphasizes the fact that with the knee flexed the action of the sartorius muscle gives rise to internal rotation and the biceps to external rotation, but when on the other hand the knee is extended, rotation of the limb can only occur at the hip-joint. He adds :

'Nature has attached all the muscles which control the movements of the toes to the bones of the leg and not to the thigh, because these muscles, when the knee is bent, would, if they were attached to the thigh-bone, contract and become locked under the knee-joint, and would not be able without great difficulty and effort to work the toes.' [1]

The *biceps brachii* is described as a flexor and supinator, the *brachialis anticus* as a powerful flexor only, the *pronator radii teres* as pronator and antagonist of the biceps.[2] All these are compared with the cords of the 'trepan' which serve to pronate and supinate the hand. The ulna is characterized as 'non-rotating' in contradistinction to the radius, and it is specified that pronation and supination take place 'without alteration of the bone which is called adjutorium (humerus)'.[3] It is stated that when the arm is bent at the elbow the flexor muscles contract whilst the extensors stretch as the angle of the bend becomes more acute.[4] The three parts of the *deltoid* muscle and their functions, together with those of the *pectoralis major* and *teres major*, are correctly reproduced.[5]

The muscles of the back are stronger than those of the front, for, since one can bend farther forward than backward, more power is required to raise oneself after bending forward than backward.[6]

The topographical dissections carried out by Leonardo, of the throat and adjacent parts, are significant.[7] Of these he has made a series of drawings ranging from rough sketches to illustrations which reproduce carefully performed dissections in great detail. With these may be classed a pair of delicate silver-points where, through the thin skin of aged subjects, we discern the fossae of the throat with the underlying muscles (Plate XXVII). In another drawing Leonardo has topographically reproduced the lower section of the face, the column of the neck, the *hyoid* bone and its connexion to the *styloid* process, the larynx, *trachea, sterno-cleido-mastoid, trapezius, splenius, scaleni, levator anguli scapulae,* the

[1] Q. vi. f. 17 r. [2] Q. iii. f. 9 v. [3] Q. iv, f. 14 r. [4] Q. vi. f. 20 r. [5] Q. vi. f. 13 r. [6] Q. iv. f. 6 r. [7] Q. v. ff. 15-18, and 20.

jugular vein with its tributaries, the carotid artery, the *vagus* with the *superior laryngeal* and the *hypoglossal* nerves and what seems to be the sub-maxillary gland, though it is perhaps a lymph node (Plate xxx). It is remarkable that in these regional drawings of the human throat he should have taken his model for the hyoid bone and larynx not from a human subject but from an animal, perhaps a dog. His serial sections of the lower extremities are also noteworthy. Leonardo seems to have been the first to make topographical dissections and serial sections to illustrate the structure of the parts (Fig. 1).

Fig. 1. Leonardo's use of serial sections

Leonardo's papers on comparative anatomy also display his great skill as a dissector, and his understanding of anatomical conditions. As commentary to a finished drawing of the abdomen and lower limbs of a muscular man, he writes :

'In order to make the comparison you must draw the leg of a frog which has great resemblance to that of a man in the bones as in the muscles. This must be followed by the hare's hind leg, which is very muscular, with conspicuous muscles unimpeded by fat.' [1]

On a folio of red-brown paper, between exquisite drawings of muscular men and of the skeleton of the pelvis and the lower limbs of a man and a horse, in which some muscles are marked out by cords (Plate xxxv), Leonardo thus writes :

'The union of the fleshy muscles with the bones without any tendon or cartilage—and you must do the same with several animals and birds. Represent the man on tiptoe so that you can

[1] Q. v, f. 23 r.

more easily compare him with other animals. Draw the man's knee bent like the horse's. In order to compare the skeleton of a horse with that of a man, you must present the man on tiptoe, when portraying the bones. On the affinity, which the conformity of bones and muscles of animals have with the bones and muscles of man.' [1]

Leonardo's great dissecting skill is seen again in the most perfect way in certain figures which display the muscles and tendons on the distal portion of the leg and on the foot, together with a part of the crucial ligament as well as the vaginal sheaths of the tendons. In these figures the toes are armed with claws of some cat-like animal, perhaps a lion [2] (Plate XXX). These drawings are by no means faultless in details, since the inner and outer edges of the foot have been interchanged, but they show, by the combination of the foot of a man with an animal's claws, one of Leonardo's most outstanding traits, his highly imaginative and artistic creative spirit. The treatment of these sketches with silver-point and colour, united to the author's power as a dissector and artist, give them a peculiar charm. They are without text, and Leonardo must have felt that they conveyed a sufficiently obvious and clear meaning.

The study of the diaphragm is a subject which Leonardo pursues with especial predilection.

'It is shaped', he says, 'like a deeply hollowed spoon.[3] . . . If it were not arched so that it could receive the stomach and other viscera into its concavity it could not afterwards contract . . . and exert pressure on the intestines, and drive the food from the stomach into the intestines, nor could it help the abdominal muscles to expel the faeces, nor could it by contracting enlarge the thoracic cavity and compel the lung to expand, so that they may inspire air to refresh the veins coming from the heart.' [4]

Leonardo points out that the diaphragm has four functions, primarily it is a respiratory muscle, secondly it presses on the stomach and drives its contents into the intestine, thirdly it aids the abdominal wall in the act of defaecation, and fourthly it divides the 'spiritual parts' (the lungs and heart) from the 'natural parts' (the abdominal organs).[5] All these functions are brought into action by the rise and fall of the diaphragm. The way in which the movements of the diaphragm and the abdominal wall

[1] Q. v, f. 22 r. [2] Q. v, ff. 11–14. [3] Q. i, f. 5 r.
[4] Q. i, f. 4 v. [5] Q. i, f. 5 r. and v.

alternate, like ebb and flow, is described and illustrated by an outline drawing.[1] Leonardo states that the muscles outside the ribs (*serrati*) must regulate them when the diaphragm contracts, as it would otherwise draw down the ribs, it being attached to them at its margin.[2]

Leonardo's treatment of the cerebral ventricles clearly shows the development of his investigations from vague and groping efforts, based on ancient and erroneous views (Plate XXIX, upper figure) to his own independent study of the phenomena (Plate XXIX, lower figure). By means of sagittal and horizontal sections through the head, he sketches the cerebral ventricles as three small vesicles lying behind each other and nearly equal in size, the foremost of which is, by means of *canals* (i. e. nerves), connected with the eye and ear.[3] In all this he has simply followed earlier authors. On the same sheet is drawn a cross-section of an onion. After he has calculated the layers he successively cuts through in bisecting the head, he says:

'If you cut an onion through the middle you will be able to observe and count all the circular layers and cases which cover the centre of the onion. In the same way, if you wish to bisect a human head, you will first cut through the hair, then the skin, then the muscular flesh and the peri-cranium, then the skull, and inside that the dura mater and pia mater, and the brain, thereupon (i. e. at the base) again the pia and dura mater, and *rete mirabile* and the base, the bone.'

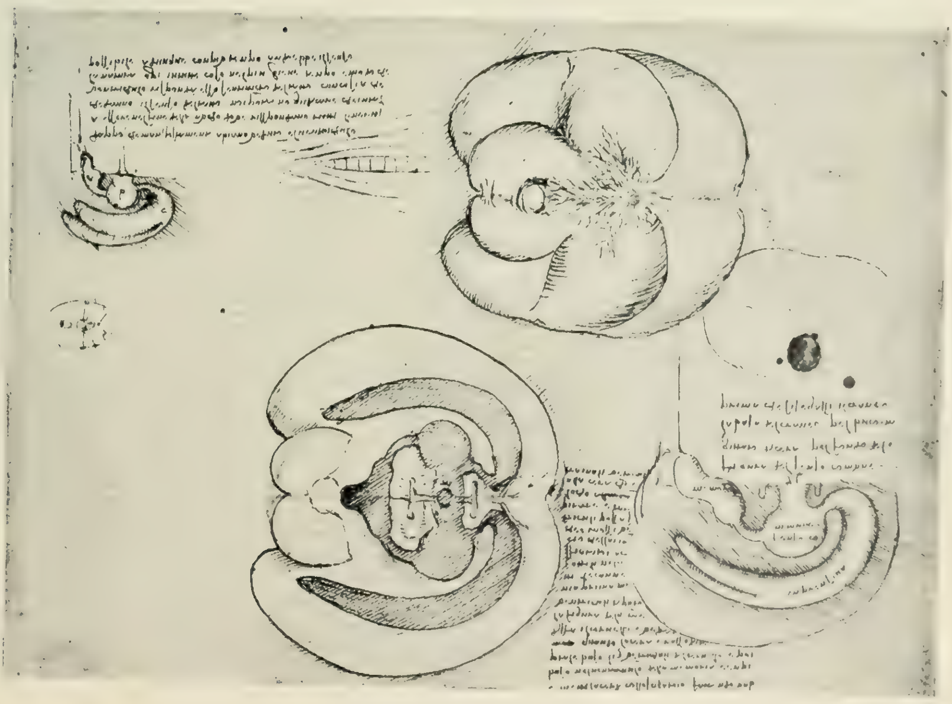
But on another folio he approaches much nearer the actual conditions and illustrates almost perfectly by horizontal sections and profile drawings the inner aspect of the base of the skull, the base of the brain, the surface of the cerebral hemispheres, and the cerebral ventricles and their relations to each other.[4] He enumerates three ventricles and the drawings show that he counts both the lateral ventricles, which are connected, as one (Plate XXIX). He designates the lateral ventricles as *impressiva*, the third cerebral ventricle as *sensus communis*, and the fourth as *memoria*; this last seems to continue downwards into the spinal cord as a thin tube (? the central canal). The fourth cerebral ventricle is mentioned as the source or meeting-point of 'all nerves which give feeling'.

In order to acquire a correct idea of the ventricles, Leonardo performed the following experiment. Into a brain removed from the cranium, he injected melted wax through a hole in the fourth

[1] Q. i, f. 6 v. [2] Q. iv, f. 1 r. [3] Q. v, f. 6 v. [4] Q. v, f. 7 r.

Quaderni V fo. 6 v

Ventricles and layers of head and eye in section



Quaderni V fo. 7 r

ventricle, having already made an opening in both the lateral ventricles and inserted a tube so that 'the air can stream out'. He then removed the brain matter from the wax so as to display the shape of the casts formed in the ventricles. He made a similar experiment with a brain without removing it from the cranium, injecting the wax through a hole bored through the base of the skull, which probably led up through the infundibulum. His words are—

'Make two air-holes in the horn of the larger ventricle and inject the melted wax into it, at the same time making a hole in the *memoria* and fill through such a hole the three ventricles of the brain; and then when the wax has hardened, remove the brain and you will see the exact form of the three ventricles. But first insert the fine tubes into the air-holes, so that the air in the ventricles can stream out, giving place to the injected wax. The shape of the sensus communis filled with wax through the hole M at the bottom of the basis cranii, before the cranium was sawn through' (Plate XXIX).

These operations of filling the soft brain cavity with a solidifying substance are fraught with many difficulties, and it is not an easy matter to get casts true to nature. As is to be expected, therefore, the figures show certain deviations from the actual state of the parts. He was, however, the first to conceive the idea of injecting a solidifying substance into the cerebral cavities, and he was the first to give a fairly correct representation of those cavities. Yet as recently as the twentieth century the claim of priority in this method has been made[1] for a modern investigator, though it was in use nearly four hundred years before his time.[2]

On several pages in the Quaderni are drawings of a number of cerebral nerves and of the spinal cord.[3] In one such drawing of the base of the brain we see the olfactory nerves, and behind them the optic tract with chiasma and optic nerve and bulbs, behind these again are shown branches to the superior maxilla from the trigeminal, next the vagi, and farthest back the spinal cord. Elsewhere the vagi are sketched in their length, and shown passing from the thorax into the abdomen, where they obviously

[1] Cf. Regius on the Raube-Welche drawings, *Biologische Untersuchungen*, 1911.

[2] Cf. Holl, 'Leonardo da Vinci', in the *Archiv für Anatomie und Physiologie*, 1911.

[3] e. g. Q. v, f. 8 r.

ramify.[1] On other pages are seen the hypoglossal, and the vagus, with the superior laryngeal.[2] Leonardo often mentions the inferior laryngeal, *nervi reversivi*, as he calls them. 'The recurrent nerves are bent upwards only because they would be torn asunder in the great movement which the neck makes in extending itself forward and further because it partly carries with it the trachea and such nerves.'[3]

In one outline drawing Leonardo probably intended to represent the medulla oblongata.[4] To this sketch, indistinct and obscure, are attached some passages which show that he had grasped the importance of the spinal cord by experiments on frogs. 'The frog retains life for some hours after the head, heart, and intestines are removed. And if you perforate this cord, it immediately shrivels and dies. All the nerves of the animals derive from here. And if you perforate the said string it suddenly twitches and dies.' And on the back of the sheet the description of these experimental investigations continues thus: 'The frog instantly dies when its spinal cord is perforated. And formerly it lived without head, without a heart, or any entrails or intestine or skin. It thus seems that here lies the fundamentum of motion and of life.' Similar outline drawings are found in others of Leonardo's manuscripts.[5]

Both in the Fogli and the Quaderni, a cord-like structure is seen on either side of the spinal cord stretching down from the brain to the foramina in the transverse processes of the vertebrae. There are several connexions shown between these cord-like structures and the spinal cord, and they are also connected with the brachial plexus. These structures are products of his imagination: they cannot be the vertebral arteries. It may be that an explanation of them would result from a further investigation of his sources.[6]

In the Fogli much of the peripheral nervous system is correctly reproduced, but in the Quaderni it is on the whole cursorily treated. The brachial plexus can be traced sometimes to the elbow, sometimes to the wrist and hand.[7] The lumbar plexus is seen to consist of three lumbar nerves from which issue the femoral

[1] Q. i, f. 13 v. [2] Q. v, ff. 16 r. and 17 r. [3] Q. i, f. 13 v.

[4] Q. v, f. 21 r, fig. 5. [5] e. g. Fogli B, f. 4 r. and v. and f. 23.

[6] Cf. Holl, 'Leonardo da Vinci: Quaderni d'Anatomia', v and vi, *Archiv f. Anatomie und Physiologie*, 1917.

[7] Q. v, ff. 19 r., 21 r. and v.

PLATE XXX


Quaderni V fo. 16 r

Dissection of triangles of neck

Quaderni V fo. 11 r

Dissection of foot. Nails replaced by claws.

nerve with a branch to the inside of the leg besides a number of other branches,[1] and the sacral plexus is represented with branches of the ischiadic nerve distributed to the pelvis,[2] high up on the thigh [3] and above the knee.[4]

Leonardo devotes a large portion of his researches to the consideration of the lungs and respiration. His portrayal of the lungs seem to be based on examination of animals [5] as well as of man.[6] He knows the shape of the lungs and their lobes.[7] Their substance is dilatable, extensible and spongy, and they are enclosed in a delicate membrane (the pleura) which interposes itself into the spaces between the ribs when they expand.[8] He shows how the pleura covers the inner side of the ribs and the surface of the diaphragm [9] and he discusses if there be air in the pleura. ‘ Whether between the lungs and the chest, at any part, a quantity of air interposes itself or not.’ [10] Later he decides against this view ‘ because there can be no vacuum in nature, the lung, which touches the ribs on the inside, must follow their dilatation ’. [11]

The bronchi ramify in the lung substance, and gradually diminish until they become blind tubes which develop minute vesicles when inflated. These endings are drawn and described, both inflated and empty, ‘ *trachea minima* uninflated and again inflated which redoubles its capacity in its increasing ’,[12] and throughout the lungs the bronchi are accompanied by the blood-vessels and the finest ramifications of the bronchi are in close touch with the most minute branches of the blood-vessels.[13] Whilst in many of Leonardo’s drawings the two bronchi enter the lung near its apex, others show approximately the actual conditions (Plate XXXVI), and here he says, ‘ first describe the entire ramification of the trachea in the lung, and then the ramifications of the veins and arteries each separately, and then all three together ’.[14]

On the findings of his experiments with inflation of the lungs, Leonardo considers it impossible that air, as such, can reach the heart from the bronchi ; on the contrary he holds that it is the pulmonary arteries that receive ‘ the freshness of the air ’ from the bronchi. ‘ To me it seems impossible that any air can penetrate

[1] Q. v, ff. 9 r., 20 v., and 21 r. [2] Q. iv, f. 9 r. [3] Q. v, f. 9 v.
[4] Q. v, f. 15 r. [5] e.g. Q. ii, f. 1 r. [6] e.g. Q. ii, f. 7 v.
[7] Q. iii, f. 4 v, fig. 7. [8] Q. ii, f. 1 r. [9] Q. iv, f. 3 r.
[10] Q. ii, f. 7 v. [11] F. A, f. 15 v. [12] Q. ii, f. 1 r. and f. 2 r.
[13] Q. ii, f. 1 r. [14] Q. iii, f. 10 v.

into the heart through the trachea (i. e. the bronchi) for he who inflates these does not expel any air from any part of these, and this occurs because of the dense panniculo, with which the whole ramification of the trachea is coated, which ramification goes on dividing into the minutest branches together with the minutest branches of the veins.' [1] And 'the lung is unable to transmit air to the heart . . . and further the air which is inhaled by the lung continually enters dry and cool, and leaves moist and hot. But the arteries which join themselves in continuous contact with the ramification of the trachea distributed through the lung are those which take up the freshness of the air which enters such lung.' Leonardo thus represents the bronchi as a tubular system, terminating blindly, from the expanded ends of which the inhaled air passes to the pulmonary blood-vessels. 'The dilatation of the lungs occurs in order that the lungs may inhale the air with which the veins which the heart sends into them can refresh themselves.' [2]

Leonardo frequently affirms that the diaphragm is the most essential respiratory muscle; but in deep inhalations such as a yawn or sigh, the contraction of the diaphragm is insufficient; then the serratus posticus superior comes into action. This he describes as made up of six muscles, three on either side, which stretch from the vertebral column to the uppermost ribs. The mode of action of these muscles is demonstrated by levers. [3] He further states that the scaleni and the serratus anterior are inspiratory muscles, and that they prevent the diaphragm from drawing the costal cartilages inwards. This is also illustrated by drawings. [4] Both in text and drawings he defines the internal intercostal muscles as expiratory, stating that they proceed obliquely upwards and forwards, that the external intercostal muscles are inspiratory and run in the opposite direction, and that the intercostal nerves, arteries and veins pass between the ribs. [5] The thorax expands on account of the oblique disposition of the ribs and of the bending of the costal cartilages, [6] the lower ribs move more than the upper; but in the case of irregular breathing Leonardo draws attention to the intervention of the abdominal muscles with action on the intestines, which again act on the diaphragm. [7]

Light sketches appear of the oblique plane of the superior

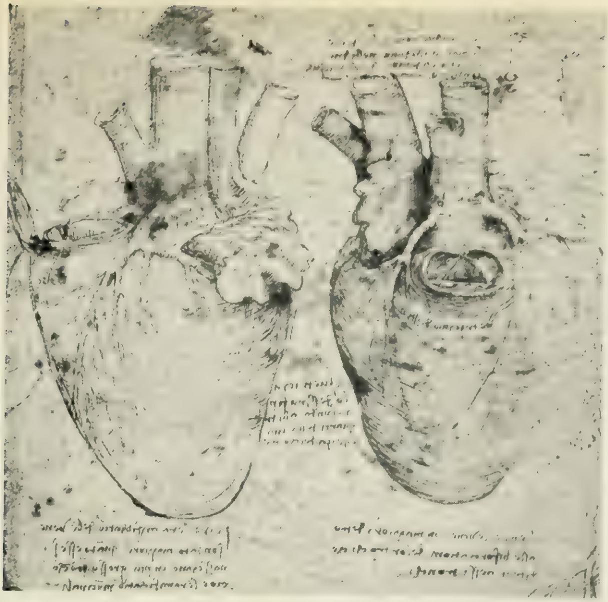
[1] Q. ii, f. 1 r.

[2] Q. i, f. 4 v. The old 'vena arterialis' = arteria pulmonalis.

[3] Q. i, f. 2 v. [4] Q. i, ff. 5 r. and 8 r. [5] Q. iv, f. 9 r. [6] Q. ii, f. 6 v.

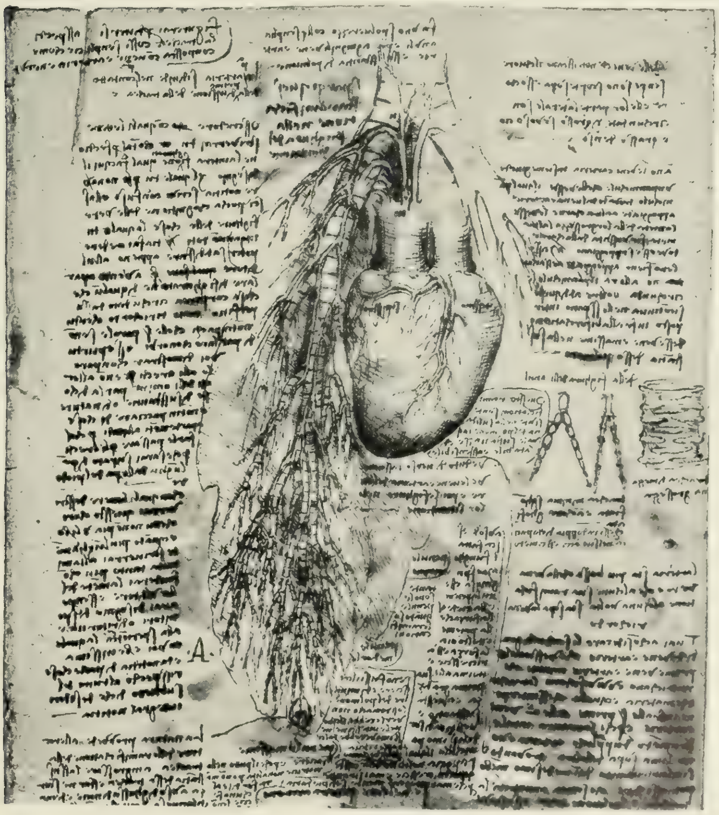
[7] Q. ii, f. 16 v, p. 35.

# PLATE XXXI



Quaderni II fo. 3 v

Dissection of coronary vessels



Quaderni II fo. 1 r

Dissection of bronchi and bronchial vessels

thoracic aperture, with the union of the costal cartilages with the sternum,[1] and also of the cartilages of the lower ribs which form the costal arch and are ranged one below the other, like a part of a cable, so that the skin may more easily glide over the cartilages when they move. The ribs are ‘ pivot-shaped ’ at their attachment to the vertebral column for the benefit of the respiration.[2]

Leonardo has observed that the shoulders move in breathing but ‘ the raising of the shoulders does not always force the lungs to inhale ’. The thorax is the receptacle for the spiritual organs, the abdomen for the natural. The lungs expand and contract continuously in all directions, but mostly downwards.[3] Leonardo has observed a calcified focus in a lung and meditates—as this unique observer always does when he meets anything unknown to him—over the causes of the process.[4] Emphasizing the recuperative power of Nature, he concludes that Nature prevents a break in the bronchi, by the thickening of the substance which becomes cartilaginous, and forms an ‘ incrustation like the shell round a nut ’. Inside the focus is found ‘ dust and watery humor ’. From the passage containing this description, he draws a line to the focus on the drawing. In another manuscript which also treats of the lungs he remarks that ‘ dust is injurious ’,[5] so that he seems to have fixed on the idea of diseases of the lung as originating from this cause.

Leonardo points out that the cartilaginous ring of the trachea, which is elastic as a spring, is incomplete at the back, where the oesophagus enters.[6] ‘ But the trachea contracts in the epiglottis in order to condense the air which seems animated from the lungs in order to form various tones of voice.’[7] He considers that the relation of the trachea to the formation of the voice must be studied and he describes which and how many muscles act on the larnyx in phonation.[8] ‘ And thus, you must not give up this study of the voice and of the trachea and its muscles until you have acquired full knowledge of all the parts contiguous to the larynx and of their functions made by nature for the modulation of this voice. And of all this you must make a special drawing, sketching and discussing the various [9] parts.’ Then resuming his experiments he deals with various phonetical problems.[10] He now busies himself with the development of sound and shows, both in text and

[1] Q. ii, f. 6 v. [2] Q. iv, f. 1 r. [3] Q. iv, f. 3 r. [4] Q. ii, f. 1 r.
[5] Q. iii, f. 1 v. [6] Q. 1, f. 9 r. [7] Q. i, f. 5 v. [8] Ibid.
[9] Q. i, f. 9 r. [10] Q. iv, f. 10 r. and v.

drawings, that the palate divides the air current, so that it goes partly through the nose, partly through the mouth. He describes the position of the lips and the part played by the soft palate in the formation of vowels and arranges these in conjunction with various consonants in tables. He compares the trachea to an organ-pipe, and thinks that the voice can be modulated by its lengthening and shortening, its expansion or contraction.

Although in other manuscripts Leonardo has reproduced the larynx very clearly, it is uncertain how exactly he understood its function.[1] He does not use the word larynx, but calls it sometimes the 'trachea', sometimes the 'upper part of the trachea', or its '*ring*', sometimes the 'epiglottis' or the 'fistula or flute, i. e. the place where the voice is formed'.[2] He states that by the dissection of animals he will make an experiment of pressing the air in and out of a lung, 'contracting and dilating the fistula, the generator of their voices'.

In dealing with the trachea and larynx Leonardo also brings in the tongue, which consists of 28 or 24 muscles, but he 'notes how they transform themselves into six in their formation in the tongue . . . and further it must be demonstrated where such muscles have their origin, that is to say from the cervical vertebrae, where they join the oesophagus, and some from inside from the maxilla, and some from the outside from the side of the trachea'.[3] The tongue takes part in the enunciation and articulation of syllables, sets the masticated food in motion, cleanses the mouth and teeth, 'and its principal functions are seven, i. e. extension, retraction and attraction, thickening, shortening, dilation and straightening'. On account of its great mobility, Leonardo frequently compares it to the penis: 'but here you might perhaps argue with the definition of the membrum which receives in itself so much natural heat, that it, besides its thickening, lengthens very much'. The surface of the tongue in the cat and bovine tribes is very rough, and as an illustration of this, Leonardo relates the following: 'I once saw a lamb being licked by a lion in our town of Firenze where there are always twenty-five or thirty of them, and where they are bred; this lion removed with a few strokes the whole skin of the lamb, and, thus denuded, ate it up.'[4]

[1] Fogli A, f. 3 r.

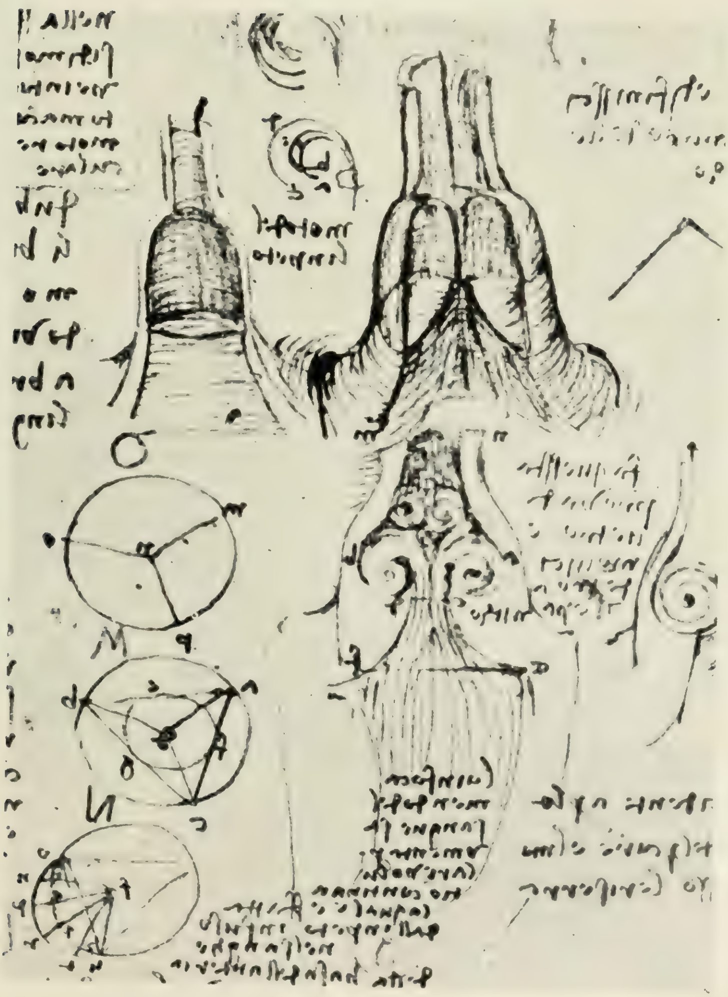
[2] See C. L. Vangensten, 'Leonardo da Vinci og fonetiken', *Videnskabsselskabets Forhandlinger*, No. 1, 1913.

[3] Q. iv, ff. 9 v. and 10 r.

[4] Q. iv, f. 9 v.

Quaderni II fo. 9 v

The Semilunar Valves



Quaderni IV fo. 11 v

Above. Glass casts with valves to illustrate action of semilunar valves.

Below. Diagrams of semilunar valves; to the right the eddies of the blood are shown.

Here again Leonardo makes a deviation in his train of thought. After dealing with the rough surface of the leonine tongue on one page, on the next his thoughts turn for a moment to Florence, for here appears a lightly sketched representation of that town [1] in the form of two inscribed circles joined by lines which go to the centre, and on which are written the names of its eleven gates.

In no less than three of the Quaderni, besides other manuscripts, Leonardo pursues, with surpassing skill, the study of the heart and the movement of the blood in it and in the larger blood-vessels. A number of drawings and paragraphs in one of the Quaderni [2] suggest that these manuscripts must have been written in his early days as an author, whilst others show that he had dissected and made much progress in comprehension of the vascular system.[3] It is obvious from these drawings that the representations of the heart are, for the most part, based on investigations of the hearts of animals, principally cattle. Occasionally however drawings of the human heart appear.[4] In several places Leonardo mentions the veins and arteries. He frequently indicates by the word ' veins ', both veins and arteries—in other words, the blood-vessels. In some manuscripts he must mean arteries when he writes ' veins ', and occasionally it is doubtful whether veins or arteries are implied. He draws the outer surfaces of the heart (Plate XXXI), and by longitudinal and transverse sections, he demonstrates its cavities, their form and projections (Plates XXXIII and XXXIV), with the trabeculae, the pectinati muscles with the depressions between the septum, the papillary muscles, the cordae tendineae, the valves, the columnae carneae, and again in a transverse section of the base [5] he shows the venous and arterial openings with their similarly disposed valves (Plate XXXIII, upper figure).

Leonardo draws the heart in the shape of a cone, with the base upwards and to the right. On the surface of the heart are seen the coronary vessels, arteries as well as veins. These ' lie together, the arteries deeper than the veins, but some of the arterial ramifications lie above those of the veins ',[6] and ' with regard to the third vein, I have not yet seen whether it has an artery with it, for which reason I shall make a dissection (really peel off the flesh) to satisfy myself '.[7] The coronary vessels are

[1] Q. iv, f. 10 v. [2] Q. i. [3] Q. ii. and iv.
[4] e. g. Q. ii, f. 14 r., fig. 1. [5] Q. iv. f. 14 r.
[6] Q. ii, f. 1 r. [7] Q iv. f. 13 v.

surrounded with fat, and covered by the pericardium which covers half the width of the vessels, the other half is covered by the flesh.[1] The arteries feed the heart's substance,[2] and spring from the aorta;[3] they 'issue from both the outer openings of the left ventricle'. By this is perhaps meant Leonardo's *hemicycles*, later known as the sinuses of Valsalva. The coronary veins are called '*vene nere*',[4] probably because they are dark in colour on account of their venous blood. Leonardo describes how the outer surface of the heart is visibly divided by the coronary vessels[5] and these anastomose at the apex.[6]

He states that the heart has four *ventricles*, two larger on the right, and two smaller on the left. The two lower ones lie in the heart's substance, and the two upper ones outside it. He opposes those who say that there are only two cavities in the heart, and maintains that if one means that the two right and two left ventricles each only form one cavity, then the room and anteroom which are separated by a small door are only one room.[7] The upper or outer ventricles should in this case answer to the ante-rooms and this word is indeed often used to designate them.[8] In consequence of the fact that most of Leonardo's material on the subject of the heart is derived from animals, his outer or upper ventricles are sometimes to be taken not as atria but as auricles, and he occasionally says this himself. 'The heart has four ventricles, that is to say, two upper, called heart-ears ('orechi'), and below them the two lower ones called the right and left ventricles.'[9] He says that these *ears of the heart* are of the nature of expanding pockets, so as to receive the percussion on the movement made by the blood when it is forcibly driven out of the ventricles when these contract, and he compares them to the bundles of wool and cotton placed on the bulwarks of a ship to soften the impact of shots from enemy bombardment.[10]

It seems reasonable to assume that Leonardo, who has dissected animal as well as human hearts, often meant atria when he used the words 'upper ventricles', and 'ears'. This is also obvious from a drawing and observation he made of an open *foramen*

[1] Q. ii, f. 1 r. [2] Q. ii, f. 4 r. [3] Q. ii, f. 3 v. [4] Q. ii, f. 4 r.
[5] Q. iv, f. 13 v. [6] Q. iv, f. 14 v. [7] Q. i, f. 3 r.

[8] Holl points out that Leonardo was the first to introduce the designation ventricles for the cardiac cavities: before Leonardo, various other expressions were in use, as *sinus, vacuitas*, or *concavitas cordis*.

[9] Q. ii, f. 17 v. and f. 3 v.

[10] Q. ii, f. 3 r. Here is one example of the many images in which Leonardo's work is so rich.

*ovale.* 'I have found from (a) in the left to (b) in the right ventricle a perforation which I here note to ascertain whether it also occurs in other hearts.'[1]

The muscles of the heart consist of longitudinal and transverse fibres. In all the four ventricles the interior muscles, which are adapted for contraction, are of a similar nature. The surface muscles on the other hand serve only the lower ventricles, but the upper ones have a continuous membrane, dilatable and contractable; elsewhere[2] he states that the upper ventricles could not empty themselves if they could not fold together, and if they had not longitudinal, oblique, and transverse muscles, able to contract.

Leonardo considers and represents the wall of the left ventricle as much thicker than that of the right,[3] and the apex of the heart to be formed mostly by the left ventricle[4] (Plate XXXIV, lower figure, and Plate XXXVI, left). To obtain a correct idea of the shape and contour of the heart's cavities, they must be inflated before dissection.[5] One can then find 'cells' or 'cavernosities' separated by rounded walls (i. e. the trabeculae and pectinati muscles). 'If you inflate the auricle you will find out the shape of the cells.'[6] The cavity of the heart is divided into two parts by a septum in which are pores, *meati*, for the passage of blood from the right to the left ventricle. As a rule, Leonardo makes the septum quite solid, occasionally with indications[7] of the *meati*, but these he admits he was unable to find himself, for he refers to them as invisible.[8]

*Musculi papillares*, which Leonardo calls the *muscles of the heart*, merge 'near the valves' into the cordae (tendineae), and the valves are held firmly by these cords. These *muscles of the heart* divide into two parts, each having its own cordae, and Leonardo has noticed that the cordae are fastened to different parts of the cusps of the valves.[9] He has made a very beautiful drawing of the papillary muscles with cordae, fixed to the tricuspid and bicuspid valves[10] (Plate XXXIII, upper fig., and Plate XXXIV, upper fig.).

In some of his drawings of ventricles Leonardo sketches the intraventricular moderator band,[11] which takes its origin in the septum and is attached at the base of a papillary muscle or a trabecula[12] (Plates XXXIV and XXXVI) This band he called

[1] Q ii, f. 11 r. [2] Q. i, f. 4 r. [3] Q. ii, f. 11 r.
[4] Q. ii, f. 4 r. [5] Q. iv, f. 13 r. and v. [6] Q. iv, f. 13 v.
[7] e. g. Q. i, f. 3 r. [8] Q. iv, f. 11 v. [9] Q. ii, f. 3 r.
[10] Q. iv, f. 13 r. and v., f. 14 r. [11] Q. i, f. 14 r; Q. iv, f. 13 r.
[12] Q. iv, f. 13 r.

*catena*,[1] and in his opinion it serves to prevent the heart dilating more than necessary, for without this structure the heart would draw too much blood from those vessels into which it had previously thrown it [2] (Plates XXXIII and XXXIV).

In one of his drawings the vena cava superior and inferior are distinctly seen to enter the right auricle separately.[3] The other drawings of these vessels and their relation to the heart seem to be taken from animals. To the pulmonary artery and vein he gives the old names of *vena arterialis* and *arteria venalis* respectively.

The aorta (*vena aorto* or *arteria aorto*) comes from the left ventricle.[4] 'The right ventricle has two orifices, one in the vena aorto, and when the heart dilates in the left ventricle its base contracts to close the door of the arteria aorto.' [5] The other orifice is 'the arteria venalis (pulmonary vein),[6] and goes from the heart to the lung'. Leonardo is here indefinite. He does not mention the left auricle.

On one occasion Leonardo calls the pulmonary artery 'the door of the lung'. Under the heading *On the names of the vessels of the heart* he says, in fact, 'the door of the lung, and it is called vena arterialis, and it is named *vena*, because it conveys the blood to the lung.' Leonardo has here erased these last words *to the lung*, but that he meant that the pulmonary artery carries blood *to* the lung seems obvious from the continuation of the passage: 'And it has three valves which open from within outwards (valvulae semilunares) with perfect closure, and these are in the right ventricle.' [7] The dominating position attributed by him to the aorta is seen from the following passage: 'And in the middle of the base of the heart is the source or base of the aorta, founded in the centre of the heart's base, having power over the state of this heart's base as the latter has power over the animal's life.' [8]

Leonardo deals often with the valves of the heart. The atrio-ventricular valves are formed, according to Leonardo, by the endocardium above and the cordae tendineae below; these may

[1] Q. ii, f. 4 v.

[2] Holl points out that Leonardo was the first to observe these fibres passing through the ventricular cavities and suggests that, in honour of their discoverer, they should be called Leonardo da Vinci's columnae carneae. Tawara in 1908 first pointed out that these fibres form bridges through which the fibres, the atrio-ventricular bundle of His, reach from the septum to the papillary muscles.

[3] Q. ii, f. 14 r.

[4] Q. ii, f. 2 v. Holl points out that Leonardo must here have written right for left.

[5] Q. ii, f. 13 v.

[6] Q. ii, f. 2 v.

[7] Q. ii, f. 2 v.

[8] Q. iv, f. 14 v.

stretch from one of the papillary muscles to two of the cusps of the valve.[1] Valvulae semilunares are portrayed both open[2] and closed.[3] Leonardo describes how the inside of the artery (i. e. either of the aorta or pulmonary artery) is covered by a thin membrane (intima) which merges into and forms one side of the valves, the other side of the valve being formed of another layer of the pannicle.[4] A similar pannicle is found in the ventricles (endocardium) and in the pericardium. The shape of the closed semilunar valves seen from above and below are so beautifully reproduced that they must have been copied after the large vessels had been filled up with a solidifying substance. Leonardo indeed here remarks : ' but first pour wax into the ports of an ox heart, so that you can see the true form of these doors.' [5]

In his opinion the valves and the roots of the great arteries are enclosed in the substance of the heart, so that the blood when it presses against the closed valves will not destroy these, but will break its impetus against the walls of the blood vessels by dilating them.[6] And in dealing with the closing of the semilunar valves when the blood passes over them, he finds, from his physico-mathematical reflections that three aorta-valves are more satisfactory than four,[7] for if their number were more than three, their angles or triangles would be weaker than those [8] formed by three valves.

It is difficult to judge how Leonardo pictured to himself the working of the heart, the movement of the valves and of the blood. He is not in the least clear in his pronouncements concerning them, and his remarks on these points are spread over many leaves of manuscript. He often says that when the ante-chambers, the upper ventricles (i.e. auricles), contract, the heart-chambers, the lower ventricles, expand, and vice versa, e.g. ' On the two lower ventricles situated in the root of the heart ; their dilatation and contraction are made at one and the same time through the flux of the flood, and the reflux of the blood is made at one and the same time, succeeding the first, through the reflux in the upper ventricles, situated above the root of this heart.' [9] The movement of the blood at the alternating dilatation and contraction of the upper and lower ventricles is thus compared with ebb and flow. By the contraction of the auricles, the blood is driven through the atrio-ventricular openings into the ventricles which open, causing

[1] Q. ii, f. 3 r. [2] Q. ii, ff. 3 v. and 4 r. [3] Q. ii, f. 9 v.
[4] Q. iv, f. 14 v. [5] Q. ii, f. 12 r. [6] Q. iv, f. 14 v.
[7] Q. iv, f. 12 r. [8] Q. iv, f. 12 v. [9] Q. ii, f. 4 v.

the semilunar valves to close. When the ventricles contract, some blood returns to the ante-chambers before the atrio-ventricular valves have closed entirely;[1] the latter when so stretched increase somewhat in size and approach one another, causing ultimately a complete closure both of the right and left atrio-ventricular orifices.[2] With the systole of the right ventricle, another portion of the blood goes through the pulmonary artery to the lungs, while a third portion goes through the septum into the left ventricle.[3] Thus less blood is driven back from the right ventricle to the right auricle than goes from the auricle to the ventricle, and the right auricle has its quantity of blood made good from the vena cava inferior, ' through the liver, the treasurer, the generator of blood '. ' The blood ', which comes to the lungs, ' gives without hindrance the necessary nourishment to the pulmonary veins, where the blood, after it has been refreshed in the lung, returns for the most part to refresh the blood left in the ventricle where it divided.'[4] What Leonardo means by this is obscure, as he does not mention through which vessels the blood returns to the heart. ' In the lungs the arteries which are connected with the minute branches of the bronchi absorb the freshness of the air entering the lungs.'[5]

Elsewhere he asks: ' Whether the pulmonary veins send back the blood to the heart when the lung contracts at the expulsion of the air ',[6] and he says that the contraction of the diaphragm compels the lungs to expand, ' which occurs in order that they may absorb the air with which the veins (vena arterialis = pulmonary artery) proceeding from the heart may refresh themselves.[7] The blood is heated in the heart's cavity, part of it evaporating (spiritus vitalis), and this vapour, mingled with dense moisture, is excreted through the farthest ends of the capillaries (' vene chapillari ') from the skin in the form of perspiration.'[8] When the left ventricle contracts, the blood goes through the aorta[9] (' the upper vessel '), and the wave of blood thus formed goes through all the arteries,[10] and with the pulsation of the blood in the heart, which closes the valves, ' a tone is created which goes through every artery and which the ear often hears in the temples '.[11]

By drawings as well as descriptions, Leonardo discusses the flow of the blood from the left ventricle through the opening of

[1] Q. ii, f. 3 v. [2] Q. ii, ff. 3 r., 8 v., 11 r., and 12 r. [3] Q. ii, f. 17 v.
[4] Q. ii, f. 4 v. [5] Q. ii, f. 11 r. [6] Q. i, f. 5 r. [7] Q. i, f. 4 v.
[8] Q. ii, f. 11 r. [9] Q. ii, f. 17 v. [10] Q. ii, f. 13 v. [11] Q. ii, f. 3 r.

Quaderni V fo. 14 r

Details of cardiac anatomy

Quaderni IV fo. 8 r

the aorta and its branches.[1] The semilunar valves open at the ingress of the blood and close with its withdrawal. He theorizes as to how far the arterial ostia open only with the central part of the semilunar valves, and to what extent the openings of the heart could have closed by mere muscular action without valves: he comes to the conclusion that the closure of the heart's openings proceeds both better and more quickly with the aid of the valves than if it occurred by action of the heart's substance.

When the left ventricle contracts, the blood, as already stated, flows into the aorta; its speed is there varied proportionately to the calibre of the vessel. When the wave of blood enters the aorta, the centre part of the wave which goes directly upwards is higher than the sides, the impetus of which dissipates itself by the lateral motion. Leonardo proves that in this the blood acts like other fluids, demonstrating both by words and drawings the action of water when it runs out of a vertical and a horizontal tube.[2]

During his deliberations over the movement of the blood in the *ostium aortae*, and above its semilunar valves, Leonardo made several experiments with models. His wax castings of the heart have already been mentioned,[3] and in this connexion he says: 'A form of gypsum to be inflated, and a thin glass within, and then break it from head to foot'; and: 'The form of the glass, to see in the glass what the blood does in the heart when it shuts the openings of the heart'.[4] In order clearly to understand the movement of the blood in the heart, Leonardo thus first made a wax cast of the ventricles and their vessels, over this he made a gypsum cast, and from this a glass cast. Through this glass cast he has examined the vortices made by the blood when it is driven out by the systole into the aorta and pulmonary artery, as shown in some of his drawings[5] (Plate XXXII). The semilunar valves close during these vortices, and the walls of the blood-vessels protrude into the 'semiventricles' or 'hemicycles' (sinuses of Valsalva).

Leonardo, however, finds it difficult to gauge to what extent this actually occurs. He says: 'It is doubtful whether the percussion caused by the forcible movement in the front of the upper arch of the hemicycle divides into two parts, of which one goes upwards and the other backwards, and this doubt is subtile

[1] Q. iv, f. 11 r. and v, f. 12 r.
[2] Q. iv, f. 11 r.
[3] Q. ii, f. 12 r.
[4] Q. ii, f. 6 v.
[5] Q. ii, ff. 12 r. and 13 v.

and difficult to elucidate.'[1] He then makes another experiment: 'Make this experiment in a glass and move . . . the panniculae (i.e. the valves) about in it '[2]. And now his doubt has vanished; he has completed his experiments and has come to the following conclusions: When the blood enters the hemicycles, it strikes the wall of the aorta and divides at the topmost edge of the hemicycle into an ascending and a descending stream. The descending part makes a spiral curve, follows the concavity of the hemicycle, and percolates through its base; it then follows the surface of the semilunar valve, stretches the valve and closes it against the other valves, the stream then turns upwards in a retrograde movement and ends in a reflex vortex.[3] The ascending part of the blood-stream also makes a whirling motion, but in the other direction. This vortex again forms other vortices, which gradually decrease until 'the impetus consumes itself'. When the blood by the systole of the left ventricle is driven through the mouth of the aorta, it strikes against the blood over the semilunar valves, and 'this concussion shakes all the arteries and pulses distributed throughout the body'.[4] The systole collapses simultaneously with the concussion of the apex and the thorax, also with the heat of the pulse, and the entry of the blood into the auricle[5] (Plates XXXII and XXXIV).

One must investigate the relations of the recurrent nerve to the heart, he tells us, to see whether this nerve lends movement to the heart, or whether the heart's movement is spontaneous.[6] Its dilatation and contraction are spontaneous, he concludes, and these movements occur on the heart's longitudinal axis.

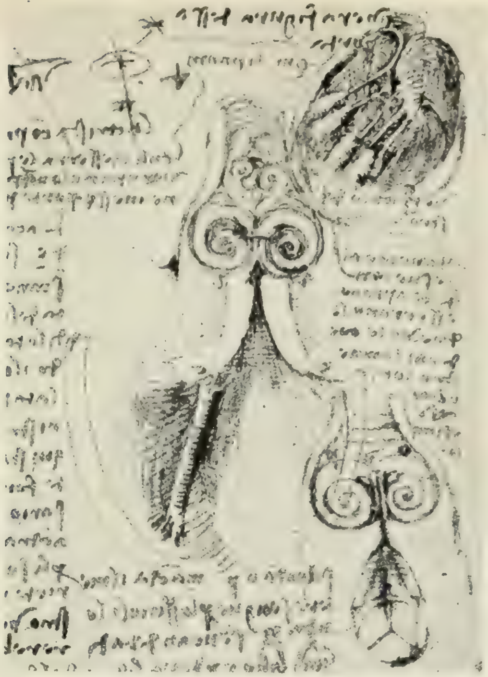
In the ventricular hollows, between the trabeculae and the pectinati muscles, to which we have already referred, the blood is driven round in a whirling movement,[7] and because it does not meet any edges or corners this movement acquires 'its vertiginous impetus', which causes heat, and this can become so great as to cause suffocation. Leonardo says that he has witnessed such an occurrence in the case of a man whose heart 'broke' as he fled from the enemy, a blood-stained sweat exuding from all the pores of his skin. Leonardo's thought then turns to the general and vital purpose of heat, and he states: 'And so heat gives life to all things, just as one sees that the warmth of the hen and turkey hen gives

[1] Q. ii, f. 13 v. [2] Q. iv, f. 11 v.

[3] It seems, to judge from fol. 11 r., par. 11, and fol. 11 v., par. 11, that Leonardo means the sinus of Valsalva. [4] Q. iv, f. 11 v.

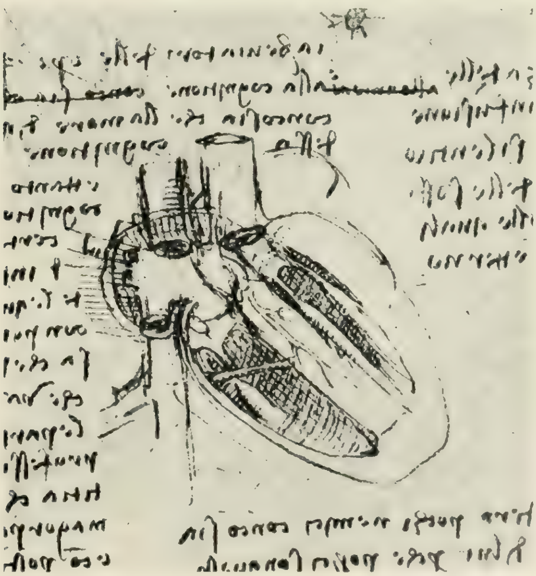
[5] Q. iv, f. 11 r. [6] Q. iv, f. 7 r. [7] Q. iv, f. 13 r., par. ii.

PLATE XXXIV



Quaderni II fo. 12 r

Right ventricle pulmonary artery and *musculi papillares.* The eddies of blood are shown around the semilunar valves.



Quaderni II fo. 14 r

Ventricles, right auricle, and great vessels

life and birth to their chickens, and the sun when it returns gives life and blossoming to all fruits.'[1]

Leonardo endeavoured also to study the movement of the living heart. He relates that when pigs were killed in Tuscany, the animal was turned on its back, fastened securely, and an instrument called a 'spillo', used to draw wine from casks, is thrust into its heart.[2] He observes that if it enters the heart the instrument begins to move: at the diastole its point goes upwards, and its handle downwards, with the shortening of the heart,—the contrary with its lengthening. At last, when the heart has ceased to move, the handle becomes stationary in the exact middle of the two extremes. During the experiment Leonardo estimated the length of the movements. 'And I have seen this several times and taken such measurements, and left such an instrument in the heart until the animal was cut up.' Leonardo demonstrates the experiment by drawings, and points out that the movements of the instrument do not continue equal in extent, giving reasons for this (Fig. 2).

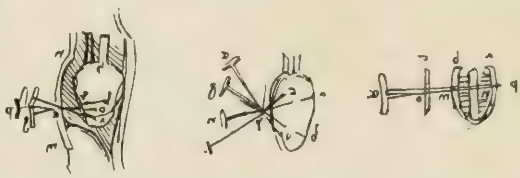


FIG. 2. An experiment of Leonardo on the heart.

Of all the bodily movements Leonardo evidently found that of the heart and vascular system the most perplexing. To this question he returns again and again, expending on it much of his time, his art of dissection, his keen observation, and his knowledge of the laws of physics. He seeks to elucidate this theme with question and counter-question directed from every point. He controverts actual authors and fancied antagonists, and his drawings, experiments, and deliberations tell their tale of his unceasing efforts to reach the ultimate truth concerning this abstruse problem. The fact that more than a quarter of all the anatomical and physiological drawings in the six Quaderni deal with the heart and its cavities shows how intense was his concentration on this subject, and it is evident from the following

[1] It is extremely interesting to follow Leonardo's train of thought in this manuscript. Par. iii reaches nearly to the edge of the manuscript; it is therefore evident that he did not intend to write anything further, but his thoughts turned to the general value of heat, and he made a note of them (par. iv) in the narrow margin at the side of par. iii. From this par. iv he drew a line to the foot of par. iii to indicate the order in which they should be read. See Plate XXXVI.

[2] Q. i, f. 6 r.

passage how necessary he thought it to use drawings as well as words for the purpose of demonstration. 'With what words will you describe this heart so that you do not fill a book; and the more minutely and elaborately you describe it, the more you will confuse the mind of the listener, and you will ever require a commentator or to revert to experience, which in your case has been very short, and explain few things concerning the subject in its entirety about which you wish full knowledge'; [1] and it is in connexion with this subject that he exclaims: 'Give an address on the shame, which is necessary for the students, impeders of anatomy and abbreviators thereof,[2] nay not abbreviators but destroyers should they be called who curtail such a task as this.' [3]

To judge from the Quaderni, the main results of Leonardo's research concerning the working of the heart may be stated as follows: At the contraction of the auricles the blood flows through the venous ostia into the ventricles. At the systole of the ventricles the blood goes out into the pulmonary artery and the aorta. By means of the pulmonary artery the blood goes from the right ventricle to the lungs, from where it returns 'refreshed' to the heart—by which vessels is not stated. From the left ventricle the blood is driven into the aorta and from it into all the arteries. Towards the skin it passes into the 'capillary veins'. Blood enters the right auricle by the vena cava.

Leonardo has not given any clear description of the circulation, his comments on the subject being disconnected and incomplete. He was not able entirely to emancipate himself from the old idea of the passage of the blood from the right ventricle through the septum into the left ventricle, although he himself seems doubtful about the truth of it, for he says that the pores in the septum are invisible. He likewise maintains that with the systole of the ventricles, some blood returns to the auricles until the atrio-ventricular valves have closed entirely. On the other hand, one must remember that Leonardo has not given any systematic exposé of the subject; his remarks are distributed over many years and many manuscripts. Nor is it surprising that Leonardo, who was no physician, and who occasionally fell short in the matter of proportion, of which as an artist he made daily use, should be incomplete and obscure in dealing with one of the most difficult physiological problems. It is also to be noted that Leonardo did not seek to publish the results of his research—it is only comparatively recently that they have been found in

[1] Q. ii, f. 1 r. [2] Q. i, f. 4 v. [3] Q. i, f. 4 r.

PLATE XXXV

Quaderni V fo. 1 r

The 'Vessel-Tree'

Quaderni V fo. 22 r

Surface Anatomy. Lower limbs of man and horse

his manuscripts. It is therefore possible, in spite of errors and omissions, that he had a fairly correct conception of the circulation. It is certain that in the Quaderni he beheld the Promised Land—that he began to enter it seems evident when one compares the Quaderni with the following statements in the Fogli :

'By the ramification of the veins in the mesentery, the food is drawn from the corruption of the aliments in the intestines, and eventually it returns by the ultimate ramifications of the artery to these intestines . . .' ; and again, 'the origin of the sea is the contrary of that of the blood, for the sea receives in its bosom all the rivers, which are produced only by the vapours of water, risen into the air : the sea of the blood is the cause of all the veins. The aorta is only one which subdivides into as many principal branches as there are principal parts to be nourished, branches which continue to ramify *ad infinitum*.'[1]

From this it may be inferred that Leonardo came very near to the conception of the circulation of the blood.

The drawings of blood-vessels in the Fogli are so beautiful that Leonardo must have prepared the vessels by injections. Many of those in the Quaderni date from an earlier period. A large and very fine full-page drawing in one Quaderni,[2] named the 'Vessel-tree' and 'Spiritual Parts', is, however, not very accurate, although the three great blood-vessels springing from the arch of the aorta are correctly rendered (Plate XXXV). His advice on the study of the blood-vessels is, 'Bisect the heart, liver, lungs, and kidneys, so that you may be able to portray the complete ramification of the blood-vessels'. Below a very rough sketch of the vascular system he writes, under the heading *Anatomia venarum* : 'The vascular system must be treated as a whole as Ptolemy represented the world in his cosmography. Later the blood-vessels of each part must be described separately and from various aspects. Study the ramifications of the blood-vessels from the back, the front, and the sides, otherwise you cannot give the true information as to their ramifications, form, and position.'[3] Elsewhere he gives a beautiful and quite correct representation of the subcutaneous

[1] Fogli A, f. 4 r. Here is written 'la vena', but there is no reason why *vena* should not here mean vessel, that is *aorta*, as it does in Q. i, f. 1 r., for example, where Leonardo calls the abdominal aorta and the vena cava inferior *le vene massime*, and in Q. ii, f. 2 v., where he deals among other things with the aorta under the heading '*de nomi delle vene del chuore*', which is translated as 'On the names of the vessels of the heart'. In the same place Leonardo calls the aorta 'Vena aorto'.

[2] Q. v, f. 1 r.

[3] Q. v, f. 2 r.

veins of the groin.[1] One sees here the union of the *vena saphena magna* and the *vena femoralis*, which latter is depicted as lying mesial to the arteria femoralis. To this figure the following passage is subjoined: 'From the soft parts (anguinaie) of the arms and thighs go veins, which, branching from the main veins, traverse the body between the skin and the flesh. And remember to note how the arteries part from the company of the veins and nerves.' The *vena dorsalis penis* also appears on the drawing, and the remark: 'There are two kinds of vein ramifications, simple and compound; it is the simple one which continues dividing itself indefinitely. A compound vein is one which is formed by two branches, as one sees n.m. and m.o. branches from two veins which unite at m. and form the vein m.p. which goes to the penis.' This sketch of Leonardo's, like many others of his anatomical drawings, might well be placed in a modern text-book (Plate XXXIII).

One would expect Leonardo's study of proportion[2] to be discussed in *Trattato della Pittura*, a work which is composed of extracts from various of his manuscripts, but it is not. In that work the fact that Nature never makes two individuals exactly alike is emphasized, the inference being that one cannot draw all one's figures from the measurement of a single subject. Only a few special measurements appear in this book. The manuscripts constituting the opening section of one of the Quaderni[3] contain the most important part of Leonardo's studies of the proportion of an adult.[4] These are sometimes rather obscure, some of the points of measurement being represented by letters

[1] Q. iv, f. 8 r.

[2] The study of proportion is the teaching concerning a harmonious relation between the body and its parts stated in figures formulated for practical as well as artistic requirements. From ancient times artists have sought a basic measurement, a module, by which they could establish a standard, i. e. the normal length and breadth of the body and its parts, and the schedule used has been called a 'canon'. The length of the head, the face, the hand and the foot, have all been used for the purpose, and the length of the body and the limbs deducted from these. The Egyptian canon seems to have been calculated from the length of the middle finger, the length of the body being 19 middle fingers. Judging from the Doryphorus of Polycleitus, the Greek canon may be taken from the Egyptian, or as some think, from the length of the head which should go eight times into the length of the body. With Albrecht Dürer the length of the body ranges from six and a half to eight times the length of the head. According to Michael Angelo's canons the length of the body seems to be between nine and ten times the length of the head.

[3] Q. vi, ff. 1–12.

[4] Leonardo states (see Richter, *The Literary Works of Leonardo, &c.*, ii, p. 109) that he will describe the proportions of the adult man and woman, but no orderly exposition of these is known to have been written by him.

which do not appear in the adjacent sketches. He also uses for purposes of measurement points which vary in different individuals, for instance, the margin of the hair, and some of his text and drawings are difficult to interpret. He is also liable to designate the same part by different terms, thus the base of the nose is named *fine di soto del naso, principio del naso*, and again, *nasscimento di sotto del naso*.[1]

Like many contemporaries, as well as earlier and later artists, Leonardo made for himself a canon of proportion, and as a basis for this he took sometimes the length of the head, sometimes that of the face or of the foot, but he also made use of others. Thus he tells us that the entire height is four times the breadth of the shoulders, and again, that four times the breadth of the shoulders equals only the distance from the sole of the foot to the base of the nose. In one manuscript the head is the length of the hand, in another it is the face that is of this length. One gathers from such contradictions that Leonardo has measured various subjects of different heights and proportions.

It is thus by no means easy to follow Leonardo's study of proportion. The manuscripts containing this material are evidently a collection which he intended to revise, for here—as elsewhere in the abundant material that he has left behind him—he deals several times with the same subject, probably at different periods and with different models, and jots down his findings without putting them in order, so that we do not know which of his measurements are individual and which general. A *résumé* of the partially worked-up sections of Leonardo's study of proportion may, however, be attempted.

*Bases.* The height of the head is the distance from the underside of the chin to the highest point of the head. The length of the face from the lower edge of the chin to the edge of the hair. The length of the foot is from the back of the heel to the point of the big toe or second toe. The width of the shoulders is to be measured between the contours of the deltoid muscles, though occasionally he indicates the 'shoulder-joints' as points of measurement.

*The proportions of the head.* Half the length of the head is from the crown to the inner canthus, from the inner canthus to the under edge of the chin, or from the under edge of the chin to the angle of the jaw, also from the top of the ear to the crown of the head; the width of the throat from back to front is also equal to half the height of the head. The distance between the mouth

[1] The quotations are given in Leonardo's own orthography, which is not always consequent (e. g. *soto* in one place, *sotto* in the next).

N 2

and the edge of the hair, and between the chin and the nape of the neck is three-quarters the length of the head, as is also the greatest width of the face.

The *face* is divided into three equal parts, namely, from the chin to the base of the nose, from here to the root of the nose, 'where the eyebrows begin', and thence to the commencement of the hair. Half the length of the face is from the middle of the nose to the chin, quarter from the lower edge of the chin to the orifice of the mouth, from the back of the ear to the nape, from the most prominent part of the chin to the throat. The width of the mouth is also one-quarter the length of the face. From the labio-mental furrow to the edge of the hair is five-sixths the length of the face, one-sixth from the labio-mental furrow to the under side of the chin, one-seventh from the edge of the hair to the crown, and from the base of the nose to the orifice of the mouth, one-twelfth of the length of the face from the labio-mental furrow to the orifice of the mouth (Plate XXXVII).

*The height and its proportions.* The total height equals eight times the length of the head. From the edge of the hair to the ground is nine times the length of the face, three times the distance between the wrist and the top of the shoulder, four times the width across the shoulders, four times the distance from the centre-line of the body to the elbow of the stretched and abducted arm. Any of these measures equals four ells, one ell (chupido) being the distance from the elbow to the point of the middle finger with stretched arm, or the distance from the point of the elbow to the point of the thumb with bent arm. Again, the height may be expressed as six times the distance from the hair edge to the pit of the throat, 12 times the width of the face, 12 times the distance from the mouth to the edge of the hair, 15 times the diameter of the throat in profile, 15 times the distance from the chin to the eye, 16 times the distance from the point of the chin to the angle of the jaw, 16 times the distance from the chin to the inner corner of the eye, 16 times the distance from the top of the ear to the crown, 18 times from the upper bend of the throat to the pit of the throat, 42 times from the front to the back of the arm at the wrist, and 54 times the distance from the labio-mental furrow to the underside of the chin.

If one compares some of these measurements of the body with those for proportions of the head, one finds that the body varies between 7½ and sometimes nine times the length of the head.

By kneeling, the height is lessened by a quarter. When in this attitude the hands are folded on the breast, the navel is the centre-point and the elbows are on a level with the navel. In the sitting posture the lower margin of the shoulder-blades and the breast will be on the same plane and both at equal distances from the seat, and from the crown of the head, while from the seat to the crown 'will be as much more than half of the man as the thickness and length of the testicles'. In an upright position, the aural orifice, the top of the shoulder, the great trochanter, and outer ankle bone will lie in a line (Plate XXXVII).

*The proportions of the trunk.* The width of the shoulders equals the distance from the pit of the throat to the navel, and is twice the height of the head; from the navel to the root of the penis is equal to the length of the head. From the nipple to the navel is one foot, as is also the distance from wrist to elbow, and from elbow to armpit. The width across the shoulders is equal to the distance from the great trochanter to the knee and from here to the ankle. The distance between the arm-pits is equal to the width of the hips, and to the distance from the shoulder-joint to the top of the hip, and from here to the lower extremity of the buttocks. The waist lies midway between the shoulder-joint and the lower extremity of the buttocks.

*The proportions of arm and leg.* From the point of the longest finger of the hand to the shoulder-joint there are four hands or, if you like, four heads, also three feet; one foot from the wrist to the elbow, also from here to the arm-pit, and with bent elbow, the distance from the top of the shoulder to the point of the elbow, and from here to the base of the fingers both equal two heads; from the finger-points to the arm-pits is equal to the distance from the top of the hip to the knee-cap, and from here to the sole of the foot, and each of the distances is two feet; two feet is also the distance from the sole of the foot to the front of the knee bent at a right angle, and from here to the back part of the buttocks; the distance from the great trochanter to the knee, and from here to the ankle, are equal.

It is probable that Leonardo's study of anatomy first sprang from his wish to represent the surface anatomy as perfectly as possible. In this he carried out his motto 'to know is to see'. He gradually became enthralled by his work of investigation and so decided to continue until he became complete master of the subject. His success was achieved through his intuition, his

power to handle a variety of subjects, his tireless research, and his unparalleled skill as a draughtsman. He knew how to reproduce as he saw, and he saw perfectly. On account of his intimate knowledge of nearly every part of the human frame, and of his delicate and artistic treatment of them, his drawings acquired a rare and hitherto unsurpassed beauty. He excels in his many delineations of surface anatomy, and in them we see portrayed a life vigorous in all its many richly varied phases and a power of observation that pierces all the layers of the body. His plastic colour drawings of the body in different positions, of which a number on red-brown and blue-grey paper appear in the Quaderni, show clearly his greatness alike as anatomist and as artist.

Under the heading[1] *On the human shape* he asks, 'Which part of the man is it where the flesh never increases when the man gets fat, Which is the part which, when a man gets thin, never becomes emaciated with too noticeable an emaciation? Among the parts which become fat, which become the fattest? Among the parts which become thin, which become most thin? Among those men who are very strong, which muscles are of greater thickness and development?' And he continues under the heading *On Painting*:

'Which are the muscles which, in old age, or in a young person who is becoming thin, separate? Which are the parts of the human limbs where the flesh never increases on account of some quality of the fat, and where also the flesh never decreases, on account of some quality of the fat, and where also the flesh never decreases on account of some degree of thinness? What one seeks in all these questions will be found at all the surface joints, as the shoulder, the elbow, the joints of the hands and fingers, the hips, the knee, ankles, and toes, and such things which will be discussed at their places.'

The following singular remark shows Leonardo's speculative turn. He states: that Nature has disposed in front all the parts most liable to be bruised, for example, the brow, the nose, the skin. If these parts were not highly sensitive, they would certainly be destroyed by the many blows to which they are exposed.[2]

Fearless and imperious, zealous and grave, Leonardo reproaches those who scorn the mathematical sciences 'in which the true knowledge of things is contained', those who satisfy themselves with superficial acquirements, those who deliberately abbreviate authors, and those who think they can dissect the spirit of God.

[1] Q. vi, f. 22 r.

[2] Ibid.

PLATE XXXVI

Quaderni III fo. 10 v

Heart, great vessels, bronchi, &c.

Quaderni IV fo. 13 r

The intraventricular muscle band

He scorns sophists and despises human folly. He condemns those who make a god of their stomach, and the betrayers of the weak and innocent. He counsels humility and the recognition of genius, and warns against persecuting them:

'And if any one is found to be *virtuoso* and good, do not drive him away, honour him so that he will not flee from you, and retire to deserts or holes or other solitary places to escape your snares. And if any one of these is found, honour him for these, our earthly gods, merit statues, portraits, and marks of honour. But I had better remind you that you must not eat their portraits, as is done in some parts of India, where, when their portraits perform some miracle, the priests cut them up when they are made of wood, and give a piece to each inhabitant—and not without compensation; and each shaves his piece and strews it on the first food he eats; and in this way they think they have eaten his virtue, and believe that he thereafter protects them from all dangers. What think you, man, of your species, here: Are you as wise as you think? Are these things which ought to be done by men?'[1]

How his steadfast will stands out. 'Obstacles', he says, 'do not deter me; every obstacle can be overcome by resolution.' Leonardo approaches with awe and reverence the anatomical problems he sets himself, but he meets them with energy and ardour, and it is in the elucidation of these scientific difficulties that his fascinating personality, his creative imagination, most spontaneously appear; it is then that one meets the full force of this powerful mind.

An illegitimate child, disinherited by his father, hated by his step-brothers, and often misjudged by his contemporaries, who regarded him as a mystic and one possessed of many secrets which he would not divulge—he naturally felt forsaken and misunderstood. He became meditative, and he commends solitude, for it alone gives time for study. 'When you are alone, you are yourself absolutely; but if you are attended by a single companion, you are only half yourself—and if you serve two masters, giving yourself sometimes to society, sometimes to thoughts of art—then I assure you that in this you will fail.'[2]

Although Leonardo was without doubt the greatest naturalist of the fifteenth century, and must have realized the importance of the diverse discoveries he made, yet he remained an unassuming man. 'As I see', he says in a *proemio*, 'that I cannot acquire

[1] Q. ii. f. 14 r. [2] Quoted from Sirén.

any more useful or more attractive matter for study since those who have gone before me have secured all that is most useful and necessary, I shall imitate the man who, through poverty, went last to the market, and being unable otherwise to get aught else, seized those things thrown aside for their worthlessness—trivial and despised wares, the leavings of many customers. These I shall lay on my miserable pack-ass and with them wander, not through the big cities, but through the poor villages, distributing them there and earning a recompense in keeping with my deeds.' [1] This attitude may also be taken as an outcome of his loneliness. A genius, whose creative impulses never left him any peace, Leonardo hardly felt himself held by any of the common bonds which force other mortals to remain where they have settled down. He seeks service with various princes—Ludovico Moro, Caesare Borgia, Louis XII, and François I, wherever he thought he might find suitable conditions for the development of his talent and his plans, and for the appeasement of that force which continually drove him onward, his insatiable desire to learn, to understand, and to create.

Leonardo seeks, line by line, to trace the lineaments of Nature in all her moods. He has the whole always in view, regarding Nature as one. He arrives at the conclusion that there is but one Natural Law which governs the whole world, and that Law is *Necessity.* Necessity is Nature's master and guardian ; it is Necessity that makes the eternal laws.[2]

All Leonardo's study of Nature, a research which has been called a divine service—is visibly inspired by a great love for everything in Nature, from man to the meanest creature, and fortune always follows his researches, whether on the Alps at Mombosa [3] he formulates the first scientific hypothesis regarding glaciers, or whether in his study he is examining the minute parts of the smallest insect or plant.

'Great love is born of great knowledge of the objects one loves. If you do not understand them you can only admire them lamely or not at all—and if you only love them on account of the good you expect from them, and not because of the sum of their qualities, then you are as the dog that wags his tail to the person who gives him a bone. Love is the daughter of knowledge, and love is deep in the same degree as the knowledge is sure—Love conquers all things.' [4]

[1] *Codice Atlantico,* f. 119 r. [2] Richter 1135.
[3] Probably Monte Rosa. [4] Quoted from Sirén.

The man who says: 'When I think I have learned to live, (then) I will learn to die,' sees clearly the vanity of all our desires.[1]

'Man who with ceaseless longings awaits the new festive spring, the new summer, coming months and coming years—man imagines that all this lingers too long on the way, and does not perceive that it awaits his own dissolution. But', he adds, 'this desire is the quintessence, the true spirit of the elements which feed themselves through the soul, imprisoned in the human form and is always demanding to return—and I wish you to understand that this longing is that quintessence which is nature's ally, and that man is the model for the whole world.'[2]

Leonardo's significance as an anatomist judged from the Windsor manuscripts can thus be shortly summed up: No one before him, so far as is known, made so many dissections on human bodies nor did any understand so well as he how to interpret the findings. His account of the uterus was far more accurate and intelligible than any that preceded him. He was the first to give a correct description of the human skeleton—of the thorax, the cranium and its various pneumatic cavities, of the bones of the extremities, of the vertebral column, of the correct position of the pelvis and the corresponding curvatures of the column. He was the first to give a correct picture of practically all the muscles of the human body.

No one before him had drawn the nerves and the blood-vessels even approximately as correctly as he, and in all probability he was the first to utilize dissections of a solidifying mass in research on the blood-vessels. Nobody before him knew and depicted the heart as Leonardo. He was the first to describe the intraventricular moderator band. Whether and to what extent he understood the circulation of the blood is still an open question.

He was the first to make casts of the cerebral ventricles. He was the first who employed serial sections. No one before him, and hardly any one since, has given such a marvellous description of the plastic surface anatomy, nor had any one before him brought forward that wealth of anatomical details which he observed, nor given such correct information as regards topographical and comparative anatomy.

Most of Leonardo's anatomical and physiological drawings must be said to belong to the domain of science rather than of art. An *artist* has no use for any knowledge of the cerebral

[1] *Codice Atlantico*, f. 252 r.

[2] Quoted from Herzfeldt CXX.

ventricles, of the anatomy and physiology of the heart, of the situation of the nerves and deeper blood-vessels, of the ramification of the bronchi and their relation to the pulmonary vessels. The man who takes interest in such things and makes them the object of his study and research is surely a *scientist.* It was in drawing that he found the most satisfactory medium for dealing with such problems, and in these drawings he is scientist and artist alike. He has drawn even the most intimate anatomical details with the accurate objectivity of a scientist and yet with a virile sense of beauty which seems unparalleled.

Although Leonardo has not left a complete and systematic descriptive anatomy nor even dealt with all its chapters, although he has not always given an exhaustive description of such chapters as he has dealt with, and although in his descriptions we can trace his dependence on old and traditional ideas, yet in the history of science Leonardo will rank as *the first to have illustrated anatomy by drawings from the object, the first of the moderns to have treated anatomy in a methodical and scientific way by means of independent research and post-mortem dissections.* His singular form of observation and concentration, his careful treatment of the scientific problems before him, his intuitive powers and his ability draw right conclusions and prove them by experiment, all these resulted in the many discoveries which he made in human anatomy and physiology, discoveries that placed him centuries ahead of his contemporaries in knowledge and in thought.

He placed anatomy in close relation to physiology, he regarded the human organism as well as that of animals and plants as ruled by the general laws of Nature ; he is, in short, *a modern biologist in the disguise of a mediaeval artist.*

When we bear in mind that Leonardo's writings and drawings have shared the fate of his pictures—that most of them have been lost and the remainder consists of fragments only—when we further contemplate what a wealth of observation, what a sum of natural science these fragments contain, when we realize that Leonardo throughout his life was busied with numerous different occupations, then we can only marvel at the gigantic energy and genius that found time for such intimate and painstaking anatomical research of a kind foreign to the ordinary artist. No one has ever, with the same right as Leonardo, been able to apply the quotation from Horace which in a somewhat free translation

Quaderni VI fo. 8 r

Proportions of trunk

Quaderni VI fo. 1 r

Proportions of head

we find in one of his manuscripts, 'God sells us everything good at the price of fatigue.'[1]

One may ask perhaps what do we learn from Leonardo's anatomical writings and drawings? They can have had no influence on the development of anatomy, for the fragments were not collected, interpreted, and published till centuries after his death. Is it not a sad tale of labour lost, a tragedy of genius working in vain without influencing the development of science? And yet Leonardo's anatomical investigations have not been altogether vain; they will for all time stand out as a proof of what human genius is capable, and Leonardo will always remain a glorious example of an unconquerable will, of continuous and intense application to work, of reverent self-restraint, of love for created things—a glorious example whose influence on posterity has not been lost with the majority of his works. Truly he is to be regarded as a genius of the highest order that the world has ever known.

To have had the opportunity to try to track his way of reasoning, the development of his theories, his methods of research, has been a privilege which has given more pleasure and joy than one can easily express. Gratitude is the predominant and overwhelming sentiment of one who has had the opportunity of laying even a single stone to the building now being reared out of the remnants of the products of one of the world's greatest investigators and thinkers, a work of restoration which yet will for ever remain unfinished since many of its fundamental stones will be for ever wanting.

*NOTE.*—In the English translation of the manuscript of Leonardo's *Quaderni* an attempt was made to maintain some of his quaint personal style of writing. The quotations given above, however, have not been given the identical form of that publication but have been altered into a more readable English.

[1] Q. v, f. 24 r. Horace, *Satire* ix, Liber i, vers. 59:

Nil sine magno
vita labore dedit mortalibus.

The phrase is doubtless derived from Epicharmus in Xenophon's *Memorabilia*, ii. 1. 20:

*τῶν πόνων πώλουσιν ἡμῖν πάντα τἀγάθ' οἱ θεοί.*

# THE ASCLEPIADAE AND THE PRIESTS OF ASCLEPIUS

By E. T. Withington

In the ancient history of medicine there are two epochs of special interest: its apparently sudden appearance as an art, or rather the art *par excellence*, with its rules and method laid down in the Hippocratic treatises in the fifth century B.C., and the transmission of this Greek medicine in modified form to the nations of the west and north in the early Middle Ages. The obscurity which covers both processes tempted early historians to seize upon any plausible explanation, and they declared that the first had its origin in the Asclepieia or temples of Asclepius, and the second in the monasteries of St. Benedict.

It was soon objected to the former theory that there is nothing priestly about the Hippocratic writings, and that no ancient author ever calls Hippocrates a priest, or represents any physician as practising in a temple. The controversy thus begun still continues, and we are told by two distinguished authorities in the same work that:

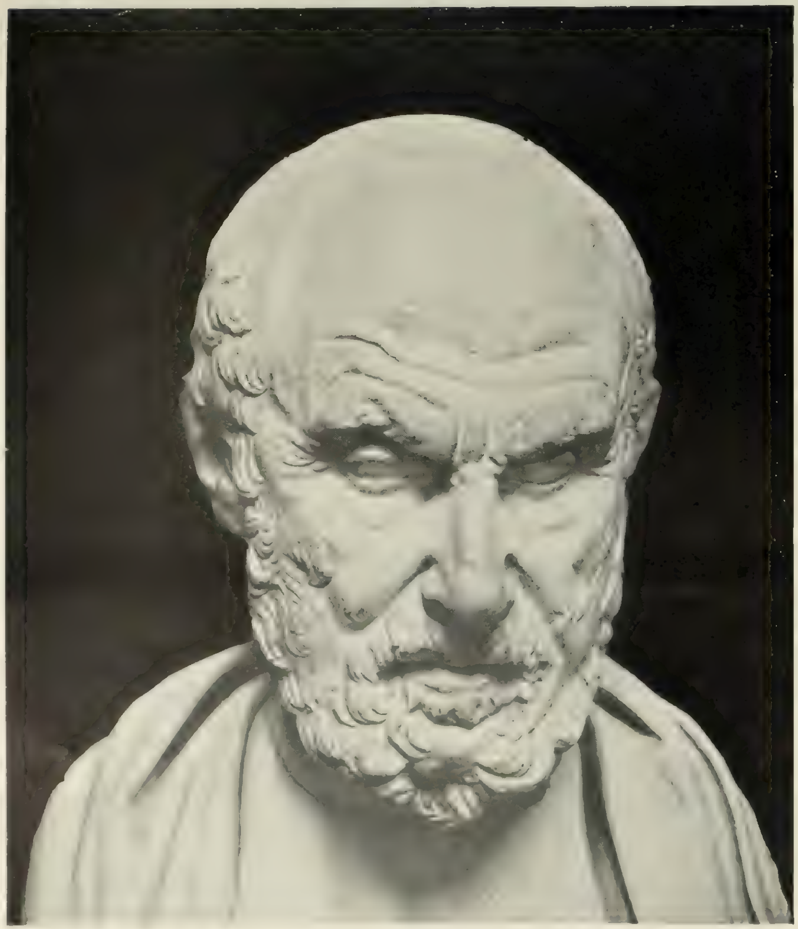
(1) 'Born of a family of priest physicians, and inheriting all its traditions and prejudices, Hippocrates was the first to cast superstition aside. . . . His training was not altogether bad, though superstition entered largely into it. . . . There is every reason to believe that the various "asclepia" were very carefully conducted hospitals, possessing a curious system of case books in the form of votive tablets left by the patients. . . . One of his great merits is that he was the first to dissociate medicine from priestcraft.'[1]

(2) 'The priests of Asclepius were not physicians. Although the latter were often called Asclepiads, this was in the first place to indicate their real or supposed descent from Asclepius, and in the second place as a complimentary title. No medical writing of antiquity speaks of the worship of Asclepius in such a way as to imply any connexion with the ordinary art of healing. The two systems appear to have existed side by side, but to have been distinct. . . . The theory of a development of Greek medicine from

[1] Art. 'Hippocrates', Sir J. B. Tuke, *Encyclopaedia Britannica*, ed. 1911.

PLATE XXXVIII

HIPPOCRATES



British Museum IInd or IIIrd cent. B.C.

AESCULAPIUS

British Museum IVth cent. B.C.

the rites of Asclepius, though defended by eminent names, must be rejected.'[1]

Sixty years earlier, Adams, the British translator of *Hippocrates*, and his French translator, Daremberg, were contradicting each other in almost the same terms. Among German authorities on Greek antiquity, von Wilamowitz opposes Thraemer, who, though less famous as a scholar, has written the articles on Asclepius in *Roscher's Lexicon* and the *Pauly-Wissowa Cyclopaedia* as well as that on Health Deities in the *Cyclopaedia of Religion and Ethics*, now in progress, and continues a stubborn upholder of the priest-physician theory. Finally, the great name of Littré, always mentioned with reverence by students of Hippocrates, must be quoted on the side of the priests, though we may hope that with our present knowledge he would have thought differently.

It is therefore not surprising that, though the priest party were supposed to have received a knock-out blow by the discovery in 1883 of the Epidaurus inscriptions,[2] with their amazing mixture of miracles, dogs, snakes, and bare-faced quackery, at the head centre of Asclepius worship, they should have rapidly recovered, and that most casual references to the subject assume rather than defend the priest theory, while a beautiful book by Dr. Aravantinos,[3] with the prestige of being written in Greek, supports the same view with copiousness and ingenuity.

It is not intended to discuss here the whole of this wide and intricate subject, but merely to contribute certain munitions in the way of arguments (some perhaps not used hitherto) to the side represented in the second extract.

The most important quotations are:

(1) *Strabo. Geography*, xiv. 2. 'They say Hippocrates was trained in the knowledge of dietetics by the cures dedicated there' (the temple at Cos).

(2) *Pliny. Natural History*, xxix. 1. 'Hippocrates is alleged to have copied out the accounts written by the patients cured in the temple of this god.' He proceeds to connect this with a story of the burning of the temple, apparently by Hippocrates himself

[1] Art. 'Medicine,' Dr. F. Payne, *Enc. Brit.*, ed. 1911.

[2] *Ephemeris Archaeologike*, 1883–5. For English translations see Hamilton, *Incubation*, London, 1906, 17 ff.; Withington, *Medical History*, London, 1894, appendix 2.

[3] Ἀσκληπιὸς καὶ Ἀσκληπίεια. Leipzig, 1907.

to hide the source of his wisdom. A similar tale had been told three centuries earlier by the physician, Andreas,[1] who, however, makes him burn not the temple of Cos, but the library of Cnidus.

(3) *Pausanias. Tour*, ii. 27, says he saw six pillars at Epidaurus 'inscribed with the names of men and women healed by Asclepius, as well as the disease from which each suffered, and how he was cured'. Two of these pillars were discovered in 1883.

(4) *Strabo*, viii. 6. 'Epidaurus is famous for the manifestation (epiphany) of Asclepius and he is relied upon to heal all kinds of disease, and has a temple there which is always full of patients and dedicated tablets on which the cures are inscribed just as at Cos and Tricca.'

The first of these is the main argument for the priest theory. Pliny is later and less reliable, and the story of the deliberate burning of the temple is universally discredited. If good for anything, it rather favours the other side, for it may indicate that no 'cures' of scientific value were to be found in the existing temple, while Andreas, who by age and profession was nearer to the facts, may have known that no temple ever contained records that could have been of much use to Hippocrates, and so substituted a library. But Strabo is a good authority, and his statement was accepted by Littré,[2] who even declared that the two treatises known as *Prorrhetics I* and The Coan *Prenotiones* are probably collections of temple records—an assertion frequently repeated.

When a great scholar makes what appears an amazing error it is well to get another great scholar to point it out. Daremberg in his translation of *Hippocrates* not only discusses the general subject, but addressing Littré on this particular point, asks, 'Why not say also that the Aphorisms are derived from these tablets; except that, from the little known of the temple inscriptions, the supposition is still more improbable? Strabo speaks of votive tablets at Epidaurus describing treatment. Now the Coan *Prenotiones* contain only prognostic statements, treatment is rarely mentioned. They have, then, nothing to do with Asclepius, nor with his priests, nor with the patients they treated'.[3]

Littré made his assertion before editing the treatises, and

[1] Soranus, *Vita Hip.*, in Kühn's *Hippocrates*, iii. 851.

[2] Hippocrates, i. 48.

[3] *Œuvres choisies d'Hippocrate*, Paris, 1855, Introduction, p. 85.

afterwards admitted himself partly converted.[1] But his original statement is still repeated by adherents of the old theory, though the treatises can now be read in several languages, and are obviously attempts to discover a natural history of diseases and their probable terminations. These, in a large proportion of cases, are disastrous—' they have a painful death ' . . . ' they are carried off quickly ' . . . ' this symptom is pernicious ' . . . ' that is fatal '. How could this sort of thing be derived from votive tablets intended to encourage future suppliants, and advertise the gratitude of the patients and power of the deity ? For, though there were doubtless sceptics in the background, so far as our records go no one (except possibly Aristophanes, the precise limit of whose ridicule is hard to define) doubted the fact of supernatural interference. It is the god who works every cure throughout the thousand years of his known activity. In the earlier period he is alleged to have cured most frequently by immediate miracles, later he often prescribed natural remedies, but the prescription remained oracular, and was admitted to be so by all writers, medical and lay. No one is ever described as going to an Asclepieion to benefit by change of air, &c., or for the cure of a disease obviously amenable to ' the art ', any more than a Catholic patient goes to Lourdes for the sake of the climate, or to get a tooth stopped. They all hoped, if highly favoured, to get an immediate miraculous cure, or, failing that, supernatural advice in a dream ; and unless we reject a vast amount of evidence, they nearly always got the dream, and sometimes the instantaneous cure.

It seems improbable that any one should even begin to study the natural course of disease, the great object of Hippocratic medicine, in conditions where supernatural intervention was continually expected.

We may add a subsidiary argument which seems to have escaped notice. The 34th Prorrhetic says, ' Muttering delirium with trembling of the hands and carphology are strong indications of phrenitis, as with Didymarchus in Cos '. Strangely enough, though many localities are mentioned in the Hippocratic Collection, this is the only patient said to have been treated ' in Cos '. But if these are the records of the local Asclepieion, they were obviously all treated there, and the remark is quite uncalled for. Further, a large proportion of the patients evidently suffered from

[1] *Op. cit.* viii. 628.

very acute disease, but our records give hardly a single case of a patient carried to a temple; they could almost all walk.

Disturbed by the discovery of the pillars at Epidaurus, later upholders of the Strabo-Littré view attempt to distinguish between pillars and tablets in quality as well as size. They even suggest that the tablets were like our hospital charts put at the head of the patient's bed, with a diagnosis of the disease and daily remarks;[1] but this is inconsistent both with the words of Strabo and all else we know of the tablets. The first case on the pillars is that of Cleo, who was safely delivered of a five years' boy, 'and he promptly washed himself and walked about with his mother. Now when this had happened to her, she wrote on a votive tablet,

"Marvel not at the size of this tablet but at the occurrence,
Five years Cleo was pregnant; she slept and the god made her whole".'

This lady evidently did things on a large scale, but even her tablet only contained a couplet. There is no reason to suppose that those of others contained more than a brief mention of how they were cured by the god, and when, in the late Roman period, we meet with something like a clinical history in the record of M. J. Apellas,[2] it requires much more than a tablet.

Still, there must be some explanation of Strabo's statement, and the following suggestion, though novel, may be worth notice. The four books 'On Diet', ascribed to Hippocrates, end with the statement that the author has investigated the rules of diet as well as he could 'with the help of the gods'. This common phrase occurs nowhere else in the Collection. It would therefore be noticeable, and might have been used by votaries of supernatural as opposed to secular treatment somewhat in this way: 'Hippocrates himself admits he got his knowledge of dietetics by the aid of the gods. He must evidently have meant Asclepius in particular, and how could he learn his treatment except from the temple cures?'

Here is a possible origin for Strabo's 'They say'—which is after all a qualification, while his statement that there was no difference between the tablets at Epidaurus and those at Cos, which the priest party now tries to distinguish, is categorical.

[1] Aravantinos, *op. cit.*, p. 164.

[2] See the Epidaurus inscriptions.

To pass to wider questions: when we try to find dates for the definite establishment of the therapeutic dream oracle they strike us as unexpectedly recent. Getting advice in dreams is, of course, as old as mankind. Some anthropologist has said that when primitive man was in a difficulty he first asked advice of his friends; if they were no good he went to the old men, and if they failed him, to those still older, the dead. He slept on the tomb of his ancestors and got advice in a dream, as Herodotus tells us was the custom of certain Africans. But for incubation in Asclepieia the oldest definite date is 420 B.C., when the worship of the god was established at Athens, and his method of cure received with ridicule by the sturdy conservative Aristophanes. This, no doubt, shows that the custom had been long in use, and there is one example of possibly earlier date in a fragment of Hippys of Rhegium, who is supposed to have flourished 'during the Persian wars', about 480 B.C. This case also occurs on the pillars, which were set up two centuries later, but the following is the Hippys version:

'A woman had a worm, and the ablest physicians gave up curing her. So she went to Epidaurus and besought the god for deliverance from the domestic plague. The god was not there, but the attendants made the patient lie down where the god is wont to heal the suppliants, and she went to rest as directed. But they, in the absence of the god, began her treatment and cut off her head. Then another put his hand down and took out the worm, a great thing of a beast, but they were no longer able to put the head back and fit it accurately to the old joining. Then the god came, and was angry with them for undertaking a task beyond their wisdom, but he himself by power invincible and divine put the head back on the trunk, and raised up the guest.'[1]

On the pillars the story is told with variations and additions. The woman was Aristagora of Troizen, and she incubated there, but Asclepius was at Epidaurus and had to be sent for—a hint that if one wants prompt supernatural aid, Epidaurus is the place.

Admitting the date of Hippys, this story not only shows the existence of lay physicians before Hippocrates, of which we have ample proof, but also indicates a clear distinction acknowledged to exist between human medicine and the supernatural aid of the Asclepieia.

It is fair to add that Dr. Aravantinos sees in this case a pretended tracheotomy by medical student deacons. Then

[1] Aelian, *H.A.* ix. 33.

enters the priest physician and administers a strong emetic or vermifuge. In the pillar version Asclepius extracts the worm after gastrotomy. The patient meanwhile is under narcosis from mandragora, or the like, and dreams of decapitation followed by gastric disturbance. Her story, though not accurate, was considered edifying, and was published accordingly. The priest physicians kept their real treatment secret and performed sham superficial operations to mislead and increase the wonder of the public.[1] There is something to be said for this, but its bearing on our present problem lies in the application.

Apart from the above, Greek literature of the fifth century has nothing to say about the therapeutic dream oracle. It is perhaps not strange that Athenian writers should ignore it, and we may notice that they always speak of medicine as a secular art. Thucydides, describing the plague at Athens,[2] contrasts the human art of medicine with prayers, divinations, and the like, saying that they were equally useless. Sophocles, in the famous ode in the *Antigone*,[3] puts the healing of diseases among the things achieved by unaided man. Even Aeschylus classes medicine with agriculture and navigation as a natural art, and if he gives it a demigod as founder, he is not Asclepius but Prometheus [4]—a myth which the Hippocratics with their emphasis on prognosis would have readily explained.

But the silence of Herodotus is remarkable. He came from the immediate neighbourhood of Cos, from a Dorian colony founded by Troizen, as was Cos by Epidaurus, though, like the Hippocratic Asclepiadae, he writes in Ionic. He was interested in medicine, giving us vivid glimpses of certain physicians and mentioning medical schools, as to which it is noteworthy that while the oldest Asclepieia are all in Greece proper, the oldest medical schools are all outside.[5] He was still more interested in the gods and their strange oracles; yet he has no word of Asclepius or the great therapeutic oracle supposed to exist close to him and to be a main source of the medicine of the age; and that though he mentions other dream oracles both among Greeks and barbarians.

[1] *Op. cit.*, p. 118. [2] ii. 47. [3] Line 364. [4] *Prom.* 460, 484.

[5] There is, perhaps, as much to be said for the suggestion that the prominence of the dream oracle in Greece proper hindered the development of medical schools there as for the theory that it gave origin to the actual schools of Cos, Cnidus, Rhodes, Cyrene, and Croton.

Most amazing is the silence of the Hippocratic writers themselves. Omitting the late and spurious 'Letters', the collection contains literally one word on the subject; the name, Asclepius, coming after Apollo in the Oath. And even the Oath gives some evidence against the priest theory, for it is taken by visiting practitioners. 'Into whatever houses I enter I will go for the good of the patients.' These Asclepiadae are great travellers (περιοδευταί), and if they settle anywhere they practise not in a temple but a surgery (ἰατρεῖον), concerning which there is a special treatise. There are also two works of great antiquity and excellence *On the Art* and *On Ancient Medicine* dealing with its nature and history. Here surely we might expect something about Asclepius, his priests and his temples, but there is nothing, nor do the writers show the least consciousness that the Art is being, or has been, freed from superstition.

This Art makes no claim to supernatural aids or inspired knowledge, but 'clearly has and ever will have its essence in causation and the power of foretelling'. In other words, relying on the uniformity of causal sequence in nature we can, from observation of like cases, foretell the probable sequence of events in diseases, and the modifications which may be produced by changes in diet and other agencies.[1]

The author of *Ancient Medicine*, perhaps Hippocrates himself, also makes diet the main source of the art, but knows nothing of temple cures. It was observed of old, he says, that if healthy men ate the coarse uncooked food of animals they were taken ill, and, similarly, if the sick followed the diet of the healthy they became worse, and combining these observations with reason men proceeded to establish by experiment the rules of dietetics. This he calls a great invention elaborated and perfected with no mean display of intelligence and observation.[2]

The healing art, he adds, 'possesses all things from of old, a principle and a beaten track along which in the course of ages many splendid discoveries have been made, and along which the rest will be discovered, if competent men, knowing the things already discovered, set out thence on further inquiries. He who, rejecting these, takes another road and claims to have discovered something is deceived and deceives himself, for it is impossible'.[3]

This idea that the method of medical progress by observation

[1] *De Arte*, § 6, Littré, vi. 10. [2] § 4, Littré, i. 580. [3] § 2, Littré, i. 572.

and reason has long been established is repeated elsewhere,[1] and though 'charms, amulets, and other such vulgarity' may be used by some, there is no suggestion that true votaries of the art have ever been deluded by these or other superstitions. What is objected to is not priestcraft, but theorizing philosophy.

The *Ancient Medicine* contains the one intimation that the art was thought worthy to be ascribed to a god.[2] We look out hopefully for Asclepius, but the writer tells us instead that this saying is justified because medical discovery depends on the application of reason to the nature of man.

Physicians are called δημιουργοί,[3] 'practisers of a public art' as in Homer,[4] and Plato, our best authority on Hippocrates, classes the physician with Pheidias and Polyclitus as an 'artist'. If you pay any of these men money they will teach you their art, medicine, or sculpture.[5] Similarly, Socrates is made to draw his comparisons with about equal frequency from the arts of the cobbler, the pilot, and the physician.

The Collection also contains a treatise *On Dreams*.[6] Certain diviners are said to be skilled in predicting from dreams things about to happen to cities and individuals, but when they try to interpret dreams foreboding bodily affections they get hopelessly muddled owing to their ignorance of physiology. The writer then makes a laudable attempt at a naturalistic theory of dreams, connecting them with bodily states, but there is not a word about therapeutic dreams. Various gods are mentioned who may be prayed to for preservation of health and prevention of disease, but Asclepius is not among them.

Diviners come off badly in these treatises. Ignorant physicians who quarrel with one another and give contradictory advice are said to bring scandal on the art, 'and almost make it resemble the art of divination, for this is how diviners act'.[7] Another treatise holds up to reprobation certain 'cheating diviners' who persuade young women recovered from hysteric affections to dedicate their best dresses 'and many other things' to Artemis, and hints that they would be better used to get married in.[8]

The forty-two clinical histories of the 'genuine' Hippocrates

[1] Littré, vi. 342; *Loc. Hom.*, § 46.
[2] § 14, Littré, i. 600.
[3] § 1, Littré, i. 570.
[4] *Od.* xvii. 384.
[5] *Protagoras*, 311 B.
[6] Littré, vi. 640.
[7] Littré, ii. 242, *Morb. Acut.* 3.
[8] Littré, viii. 468, *Morb. Virg.* 1.

correspond in number with the forty-two 'Cures of Apollo and Asclepius' on the pillars, but in everything else it is impossible to imagine a greater contrast. Most of the Hippocratic cases end fatally, and the treatment of those who recover is rarely considered worth mention. The temple cures are instantaneous or nearly so; 'when day appeared he went away healed' is the usual ending, the few instances of delay being due to want of faith or over hastiness of the attendants, as with Aristagora.

To sum up: if there was a therapeutic dream oracle at Cos in the fifth century, the Hippocratic writers will have nothing to do with it, they will not even mention it. As von Wilamowitz says, 'The Epidaurian colonists carried Asclepius with them to Cos, but not medicine. Where medicine is concerned 'the god disappears. Instead of the priest comes the physician; instead of the dream oracle, science; instead of Asclepius the Asclepiad'.[1]

A partial explanation of this remarkable silence may be found in the practical certainty that the Asclepiadae were not only not priests but not Dorians. Our earliest information about them is a chapter-heading from Theopompus. 'Concerning the physicians in Cos and Cnidus, how they are called Asclepiadae, and how they first came from Syrnus', a town in Caria. They came, then, from the East, not West, and belonged to an older stratum of Greek emigrants. They wrote in a dialect more Homeric than Doric. and held the Homeric view of Asclepius, that he was a mere man, the founder of their guild and ancestor of its more prominent members. They were, perhaps, astonished by the appearance of their patron as a Dorian earth-god followed by mystic snakes; and though they may soon have established a *modus vivendi* with the invaders, a difference of view as to Asclepius remained permanent, for both Galen[2] and Pausanias[3] tell us that it was disputed even in their time whether Asclepius was a deified man or 'a god from the beginning'.

There is evidence of this difference at Cos. The temple certainly acknowledged the primacy of Epidaurus, whence it got its snakes, but, according to another story, it was founded direct from Tricca, the home of the Homeric and purely human Asclepius. A deified hero might appear and give advice at his tomb, as did Amphiaraus, but an epiphany of Asclepius, like that of the high gods, might

[1] *Isyllos von Epidaurus*, Berlin, 1886, pp. 37 and 103.
[2] i. 22. Kühn's edit., *Protrept.* 9. [3] ii. 26.

occur anywhere, for he was also an embodiment of the great earth-spirit in his capacity of counsellor and healer. Possibly there was first a simple shrine at Cos, where the Asclepiadae celebrated the memory of their patron but did not expect him to appear; while the dream oracle and the snakes came later from Epidaurus, and were for a time deliberately ignored by the medical school.

So much for the silence of Hippocrates, or rather, of the Asclepiadae, the importance of which as regards our problem, especially when combined with what they do say, has perhaps been overlooked.

Neither Celsus nor the author of the brief outline of medical history in the *Introduction*, formerly ascribed to Galen,[1] mention priest-physicians, and the former tells us that Hippocrates separated medicine from philosophy.

For the bearing on the question of inscriptions concerning medical men and priests the reader should refer to the work of Pohl (*De Graecorum medicis publicis*, Berlin, 1905), where full references are given. The writer sums up his conclusion thus: 'In the latest age, then, the true art of medicine and that of the priests of Asclepius got mixed, after arising from widely separated sources and continuing a long time distinct.'

The slight modification we would make in this statement may be illustrated from another point of view, not yet sufficiently studied, the attitude of later medical writers towards the oracle.

We have seen that the earlier ignore it, and our first available reference is from Rufus of Ephesus, called by Oribasius 'the Great', whose fragments reveal a man of much ability and sober-mindedness.

A certain Teucer, afflicted with epilepsy, went to the Asclepieion at Pergamus and besought the god to heal him. Asclepius appeared, as usual, in a dream, and asked whether he would like another disease instead. Teucer replied, this was not his most earnest desire, in fact he would rather be healed entirely; but if that was impossible, and the other disease less troublesome, he would accept it. The god replied that it was less troublesome, and was also the best cure for his complaint. Thereupon he was attacked by a quartan fever, but was delivered from his epilepsy.[2]

[1] xiv. 674. *Introductio seu Medicus.*

[2] Oribasius, xlv. 30, Daremberg's edition, iv. 86, Paris, 1862.

This is told in illustration of the Hippocratic Aphorisms that fevers generally,[1] and quartans in particular,[2] relieve spasms, a principle which, according to Rufus, had been successfully utilized in medicine.

We pass to *Galen* with great expectations, for he was a native of Pergamus. He calls Asclepius his ancestral god (though he was not an Asclepiad), and tells us he saved his life at least once by curing a disease, and perhaps again by warning him not to go with Marcus Aurelius to the Danube. He therefore became a special *θεραπευτής* or votary of the god. Yet the vast bulk of his extant works affords us only four or five cases, including his own. This, he says, was 'a pernicious abscess',[3] but he describes it elsewhere as a chronic pain between the liver and diaphragm.[4] He was then a young man and thought he was going to die, but Asclepius in a dream recommended bleeding from an artery and he rapidly recovered. The god ordered the same treatment in the case of another 'votary' with a chronic pain in his side, and it was partly owing to these 'cures' that Galen became convinced of the value and safety of arteriotomy, of which he previously had doubts.

A third case is that of the corpulent Nicomachus,[5] whose treatment unfortunately is not given, but may be connected with a remark made elsewhere that patients will obey the directions of Asclepius even to the extent of going entirely without drink for fifteen days, which they would never do on the orders of physicians.[6]

A fourth cure is that of 'elephas', supposed to be leprosy.[7] Here the patient was summoned by the god from Thrace to Pergamus, and the treatment ordered was the internal use of *theriac*, a famous medicine containing viper's flesh, combined with viper's-flesh ointment. The disease was thereby converted into 'lepra', which was cured 'by drugs ordered by the god'. According to Galen, the treatment of elephas by viper's flesh had been discovered accidentally some time before, and had been successfully used by himself. There is also evidence that it had been employed by Archigenes half a century earlier (Aetius, xiii. 121).

[1] iv. 57, Littré, 4, 522.
[2] v. 70, Littré, 562.
[3] xix. 19, Kühn's edition, *Libr. Propr.*, 2.
[4] xi. 314 ff., *Venesect.* 23.
[5] vi. 869, *Dif. Morb.* 9.
[6] xvii*b*. 137, *In Hipp. Epid.* vi. 4, 8.
[7] xii. 315, *Simpl. Med.* xi. 1.

Finally, we have a recommendation in a dream of a local application for swollen tongue, confirming Galen's advice against that of other physicians, but this is not clearly attributed to Asclepius.[1]

In these cases the treatment, so far as known, contains an element of recognized medicine, but the theurgic side remains prominent. It is the god who cures, and the patients attribute their recovery to his supernatural interference. So, too, Rufus and Galen, as will be clearer to those who look up the context, show not the slightest doubt of the supernatural character of the intervention. They may point out the medicinal nature of the means, and some circumstances which favour success, but the cure is essentially divine even when it gives divine sanction to human methods. There is no true mixture: the 'Cures' of Asclepius are separated to the last from those of the art by a strong element of miracle: they are two distinct things, and medical writers show no consciousness that they ever had been, or were tending to become, the same. The latest recorded temple cure is as purely miraculous as the earliest, though in a different way.

Archiadas of Athens had a daughter afflicted by a painful and incurable disease. So he besought Proclus the philosopher to intercede with Asclepius for her cure. Proclus, whose benevolence equalled his wisdom, at once went to the Asclepieion, 'fortunately not yet plundered', and prayed according to the ancient rites. The patient felt a sudden change in her condition and great relief, and when the philosopher, having ended his petitions, came to see the effect, he found her 'delivered from her pains and in perfect health'. This was in the latter part of the fifth century.[2]

The closing of the Asclepieia was followed almost immediately, or even preceded, by the establishment of dream oracles in Christian shrines, especially that of SS. Cyrus and John at Alexandria. Seventy of their cures are recorded by Sophronius, Patriarch of Jerusalem (d. 640), who had himself been healed by them. They closely resemble those of Asclepius, the chief difference being that the saints are represented as indignant at any suggestion that their cures are not miraculous throughout,

[1] x. 971, *Meth. Med.* xiv. 9.

[2] Marinus, *Vita Procli.* For translation see Taylor, *Proclus*, London, 1788, i. 26. The god also appeared to Proclus on his deathbed, 'and would probably have cured him had he desired it,' *op. cit.* i. 27.

and inflict severe punishment on a rash physician who declared that some of their prescriptions might be found in *Galen* and *Hippocrates*.[1] In fact secular and sacred medicine are in open opposition.

To return to the fifth century B.C. and the origin of Hippocratic medicine. While not professing to solve that obscure and interesting problem, it is hoped that some additional reasons for rejecting the superficial view that 'it originated in the health temples' are given in these notes. What can be said in favour of that view may be found in the works noticed at the beginning. It may seem to some that the arguments there adduced do not outweigh the single fact (the most certain in medical history) that in the fifth century B.C. medicine was known throughout Greece as an art, the chief of arts, too long to be learnt in this short life. For arts are not learnt in temples by observing real or supposed supernatural intervention, but, as the Hippocratic writers tell us, by experience and the application of reason to the natures of men and things.

[1] Mai, *Spicilegium*, vol. iii, case 30, Rome, 1840.

# THE SCIENTIFIC WORKS OF GALILEO (1564—1642)

## WITH SOME ACCOUNT OF HIS LIFE AND TRIAL

Being a Review of Favaro's *Edizione nazionale delle Opere di Galileo* (1890–1909)

By J. J. Fahie

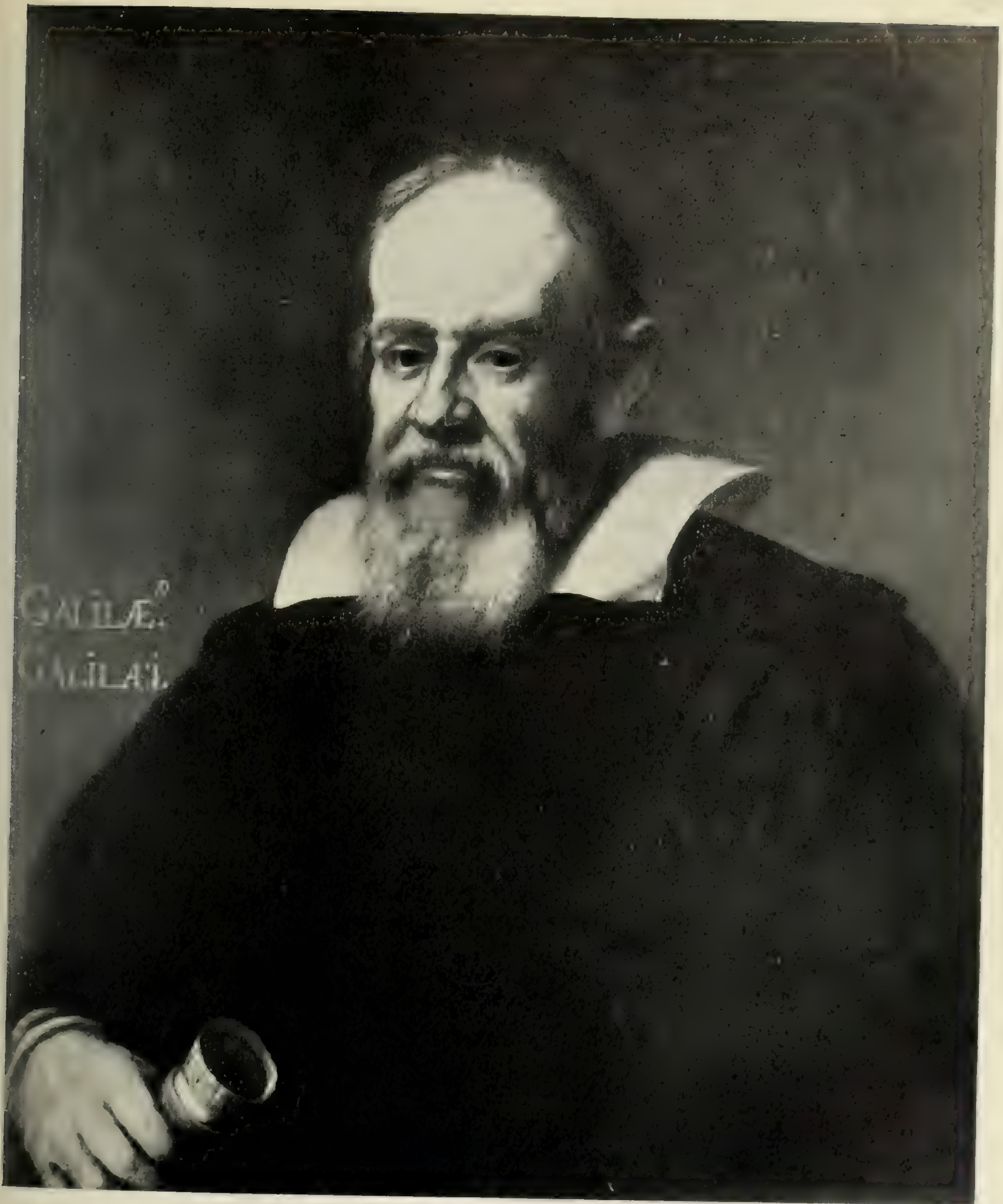


### I. The Galileian Researches of Antonio Favaro

In the current year 1920, Professor Antonio Favaro, of Padua University, completes forty-four years of Galileian studies. The result is monumental; besides editing the National Edition of Galileo's Works in twenty large quarto volumes, Favaro has published over 450 separate studies on matters relating to the life, times, and activities of the great master. The vastness of the labour involved in this kind of literary work can be appreciated only by those who have themselves engaged in historical researches.

Antonio Favaro was born on the 21st of May, 1847, in the Villa Favaro at Fiesso d'Artico, on the Brenta, and about half-way between Padua and Fusina. He was educated at the Universities of Padua, Turin, and Zürich, and, after a few years of private teaching, was elected in 1872 to the newly created chair of Graphical Statics in the University of Padua, being then only twenty-five years of age. A few years later he was appointed Director of the *Scuola d'applicazione per gli Ingegneri.* In 1877 he published his first work in Padua, his *Lezioni di Statica grafica.* This same year was destined to be of the first importance in the ordering of his life, for at this time, stimulated in the first instance by a suggestion from the poet Giacomo Zanella, Favaro began to devote himself to the study of the life and work of Galileo.

# PLATE XXXIX




Devot:me et Oblig:mo Ser:re

Galileo Galilei

From a portrait in the Bodleian brought from Florence in 1661

After four years of close study and research, chiefly amongst the 303 volumes of Galileian MSS. and papers in the National Library at Florence, Favaro came to the conclusion that a new and complete edition of Galileo's works was necessary. After six years of strenuous advocacy he had the satisfaction of seeing his ideas crowned with the highest recognition.[1] On the 20th of February, 1887, King Umberto issued a Royal Order decreeing the publication of a new and complete edition of the works of Galileo, at the cost of the State, and under the care of the Minister of Public Instruction. Favaro was appointed Editor-in-Chief, and there were nominated as his coadjutors Professor Isidoro Del Lungo, of the Accademia della Crusca, who was to occupy himself with all that concerned the care of the text, and Professors Genocchi, Govi, and Schiaparelli to assist in surmounting the scientific difficulties that might present themselves.[2] The first volume appeared in 1890, and the twentieth and last in 1909. The series represents a magnificent tribute by the Italian people to the greatest scientific genius that their nation has produced.

In our exposition of the science of Galileo, we shall follow the chronological order adopted in the National Edition, prefixing to our presentment in each case a note indicating all the places in the National Edition where the particular matter is dealt with.

We desire to express our obligations to Mr. John Murray for kindly allowing us to utilize part of the material of our book *Galileo; His Life and Work*, London, 1903.

## II. Early Manhood of Galileo, 1564–92

### 1. *Training and Education*

Galileo Galilei was born of Florentine parents at Pisa on the 15th of February, 1564. Here he passed the first ten years of his life, and he received his early education, partly at the school of one Jacopo Borghini, and partly at home, where he was helped in the study of Greek and Latin by his father, who was a good scholar and mathematician, and an authority on the theory and practice of music.

At the age of twelve Galileo was sent to the monastery of Vallombroso, near Florence, for a course of the 'Humanities'.

[1] *Intorno ad una nuova edizione delle Opere di Galileo*, Venezia, 1881.

[2] *Per la Edizione nazionale delle Opere di Galileo—Esposizione e Disegno*, Firenze, 1888.

the literary education then considered indispensable for a well-born youth. Here he made himself acquainted with the best Latin authors, and acquired a fair command of Greek. For the course on logic, as then taught, he had little taste, contenting himself with what scraps of elementary science and philosophy he could pick out of the lessons.

From early boyhood Galileo was remarkable for mental aptitudes of various kinds, coupled with a very high degree of mechanical skill and inventiveness. His favourite pastime was the construction of toy-machines or models, such as wind and water mills, boats, and other common mechanical contrivances. When unable to supply some essential part, he would adapt the machine to an entirely new or quaint purpose, never resting satisfied until he saw it work.

As he grew he imbibed from his father something of the theory and practice of music, and became skilful with the lute. He was also, it is said, a creditable performer on the organ and one or two other instruments. Music, and especially the lute, gave him pleasure through life and afforded the greatest solace in later years, when blindness was added to his other afflictions. His talent in drawing and painting was equally striking. In later life he used to tell his friends that, had circumstances permitted him to choose his own career, he would have decided to become a painter.[1] Galileo was also very fond of poetry, and his essays on Dante, Tasso, and Ariosto, as well as some verses and the fragment of a play, bear witness to a cultivated taste.[2]

In 1581, when seventeen and a half years old, Galileo was sent to study medicine and philosophy at the University of Pisa, a course which his father, who was in straitened circumstances, regarded as likely to prove lucrative.

### 2. *On the Pulsilogia*[3]

About a year after his matriculation (1582–3), Galileo made his first discovery—that of the synchronism of the oscillations of

[1] Among the circumstances which assisted the rise and progress of the reformed Florentine School, Lanzi includes ' the readiness with which the celebrated Galileo imparted to artists his discoveries and the laws of perspective '. *History of Painting*, Bohn's edition, vol. i, p. 210.

[2] These are collected in vol. ix of the National Edition of his works.

[3] Cf. Nat. Ed., vol. x, p. 97 ; vol. xix, pp. 603, 648.

the pendulum, timing the excursions by his pulse. This was the first attempt at accurate measurement of any bodily function, as well as the basis of the modern clocks. He was not, however, then thinking of clocks, but only of the construction of an instrument which should mark with accuracy the rate of the pulse and its variation from day to day. He quickly gave form to his idea, and it was welcomed with delight by physicians, and was long in general use under the name of Pulsilogia. Sancto Santorio, Professor of Medicine at Padua, was the first to give diagrams of the Pulsilogia in his *Methodus Vitandorum Errorum in Arte Medica* (Venice, 1602), three of which we reproduce (Figs. 1–3).

FIG. 1.

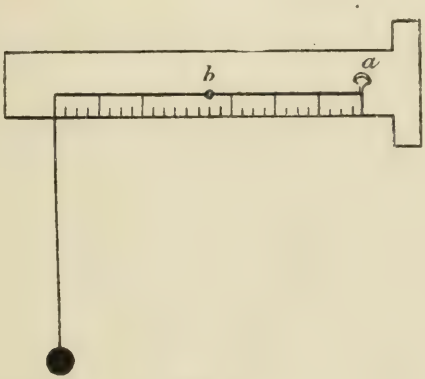



FIG. 2.

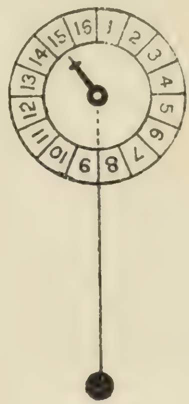



FIG. 3.

Fig. 1 shows a weight at the end of a string held at the top of a scale, which can be graduated so as to show the number of pulsations per minute. The string is gathered up in the hand till the oscillations of the weight coincide with the beats of the patient's pulse. Then, a greater length of string, i. e. a longer pendulum, would indicate a slower pulse, and a shorter length a more lively action. In Fig. 2 an improvement is made by connecting the scale and string; the length of the latter is regulated by turning the peg *a*, and a bead *b*. on the string indicates the rate of pulsation. Fig. 3 is still more compact, the string being adjusted by winding (or unwinding) upon an axle or drum at the back of the dial plate.

In many of his subsequent experiments and investigations, Galileo utilized this principle, as in his innumerable experiments on motion, his long-continued observations on the periods of Jupiter's satellites, and, just before he died, in the design of a pendulum clock.

Vincenzio Viviani, a pupil and the earliest biographer of Galileo, tells us that the young Galileo's attitude from the first in the philosophical classes was not at all to the satisfaction of his

teachers, owing to his habit of examining every assertion to see what it was worth. In consequence of this he soon acquired a reputation among the professors and students for bold contradiction, and was dubbed 'The Wrangler'. His eager questioning, especially of the dictates of Aristotle, found no favour in their eyes.

The first volume of the National Edition opens with what the editor calls Juvenilia, written in or before the year 1584, and entirely in Galileo's own hand. They are commentaries in Latin on the *De Caelo* and kindred tractates of Aristotle, and are almost entirely transcripts of lectures he had attended. Beside their personal interest these Juvenilia are of value for the light they throw on the scholastic character of the teaching then prevalent in Italy.

### 3. *Galileo and the State of Mechanics in Italy at the end of the sixteenth century*

Up to Galileo's time the study of mathematics, although included in the *Rotoli* or lists of University lectures, was practically neglected in all the Universities of Italy, though Commandino (1509–75) and Maurolico (1494–1575) had recently revived a taste for the writings of Euclid and Archimedes; and Vieta (1540–1603), Tartaglia (1500–59), Cardano (1501–76), and others had made considerable progress in algebra. Guido Ubaldi del Monte (1540–1607),[1] soon to become a warm friend and patron of Galileo, Besson (d. *c.* 1580),[2] Ramelli (1531–90),[3] and one or two others had done something towards the application of statics, the only part of mechanics as yet cultivated. Thus Guido Ubaldi's *Mechanicorum Liber* of 1577 consists of little more than bald descriptions of the mechanical powers—the balance, the lever, the pulley, &c. He explains combinations of pulleys at great length, and reduces their theory to that of the lever, but his solution of the problem of equilibrium is very erroneous. The other contemporary works on mechanics consist solely of descriptions of actual and imaginary machines, with no reference to general principles. There was, in fact, no general recognition of the necessity of applying mathematics to the study of mechanisms.

[1] *Mechanicorum Liber*, Pisauri (now Pesaro), 1577.

[2] *Theatrum Instrumentorum*, Lyon, 1582.

[3] *Le diverse ed artificiose macchine*, Paris, 1588.

To the close of his nineteenth year, Galileo knew nothing of mathematics, and appears to have been first led to study geometry by his fondness for drawing and music, the underlying principles of which he wished to understand. His father tried to prevent these studies, but the natural bent of the young man's mind was not to be denied.

During the winter and spring of 1582–3 the Court of Tuscany was at Pisa, and among the suite was one Ostilio Ricci, mathematical tutor to the Grand-Ducal pages, and a friend of the Galilei family. The tutor and the young student became friends. Going on one occasion to pay Ricci a visit, Galileo found him lecturing to the pages on some problem in Euclid. He did not enter, but, standing by the door, followed the instruction with rapt attention. This was the awakening of a new craving of the intellect, under the influence of which he found himself drawn repeatedly to the class-room. Each time, concealing himself behind a door, he listened, Euclid in hand, to the teacher's demonstrations. At last, taking courage, he begged the astonished tutor to help him, which Ricci readily consented to do. Henceforth, mathematics were more studied than medicine, for which, truth to say, he never showed any relish. For some unknown reason, however, after nearly four years' residence, and without taking the doctor's degree, Galileo withdrew from the University, and went home to Florence about the middle of 1585, where, still under the guidance of Ricci, he devoted himself heart and soul to mathematics and physics. From Euclid he passed to Archimedes, whose work in mechanics he was destined to continue, and for whom he then conceived a life-long veneration.[1]

### 4. *The Hydrostatic Balance* [2]

In 1586, when fresh from the study of the great Syracusan's work on hydrostatics, Galileo constructed a hydrostatic balance (La Bilancetta) for finding with accuracy the relative weights of any two metals in an alloy. In describing it he refers to the popular account of the way in which Archimedes detected the fraud committed by the goldsmith in the making of Hiero's crown. He doubts the correctness of that oft-told story, the results of which, he says, are fallacious, or at least little exact, and he

[1] Nat. Ed., vol. i, pp. 231–42.

[2] Cf. Nat. Ed., vol. i, pp. 211–28.

believes that his own 'most exact method' was that really employed by Archimedes himself.

Take a lever A B (Fig. 4) at least a yard long (the longer it is, the more accurate its indications) and delicately suspend it from its centre C, and at the end B let there be means of attaching the body to be tested, say an alloy of gold and silver, and its counter-weight at the other end A. First, take a piece of pure gold and weigh it in air ; now immerse it in water ; it will seem to be lighter, and the counterpoise D must be moved from A to, say, E to obtain a balance. Then, as many times as the space C A contains the space A E, so many times is gold heavier than water. Proceed in the same way with a piece of pure silver. When placed in water it will seem to lose more of its weight than

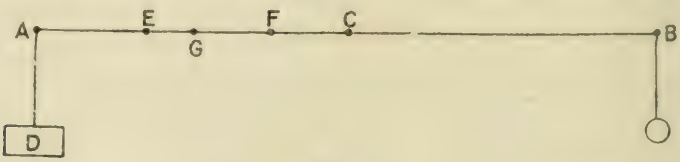



FIG. 4.

the gold, and its counterpoise will have to be moved to, say, F, showing that silver is specifically less heavy than gold in the ratio A E to A F. Now taking the alloy, it is clear beforehand that it will weigh less than an equal volume of pure gold, and more than an equal volume of pure silver. Weigh it in air, and then in water, when it will be found that its counterpoise must be moved to some point between E and F, say, G. From this we conclude that the weight of the gold in the alloy is to that of the silver as F G is to E G.

### 5. *The Centre of Gravity in Solid Bodies* [1]

After this work Galileo passed to the investigation of the centre of gravity in solid bodies. The results are embodied in an essay first circulated in manuscript, and only printed in 1638, as an appendix to the Leyden edition of his *Dialoghi delle Nuove Scienze*. His first intention was to prepare an exhaustive treatise in completion of the work of Commandino on the same subject. There chanced, however, to fall into his hands the book of Luca Valerio (1552–1618),[2] the Neapolitan mathematician, in which he

[1] Cf. Nat. Ed., vol. i, p. 182 ; vol. viii, p. 313 ; vol. xvi, p. 524.
[2] *De Centro gravitatis solidorum*, Rome, 1604.

found the matter treated so fully that he discontinued his investigations, although, as he tells us, the methods he employed were quite different from those of Valerio. This study, however, attracted the attention of the Marquis Guido Ubaldi del Monte of Pesaro, himself an able mathematician, who introduced the author to the notice of Ferdinando I, Grand Duke of Tuscany.

Late in 1587 Galileo made his first journey to Rome, probably with the object of finding there some opening, or in furtherance of his designs on the chair of mathematics at Bologna, which had been vacant since 1583. The visit led to his acquaintance with Father Cristoforo Clavio (1557–1612) of the Society of Jesus, a celebrated mathematician, to whom mainly the world is indebted for the reform of the calendar in 1582. In later years he was a stout opponent of the new astronomical doctrines of the younger man, though before his death on the 6th of February, 1612, he became one of Galileo's most distinguished converts.

Evidently while in Rome the two new friends had been discussing mathematics, for, on his return to Florence, Galileo sent a letter to Clavio, under date 8th January, 1588 (the earliest of his letters known), respecting his theorem on the centre of gravity of a rectangular conoid, or truncated pyramid. ' Those ', he says, ' to whom he had already submitted it, were not satisfied, and, therefore, he could not be so himself.' In this dilemma he solicits the learned Father's opinion, adding that, ' if it, too, was unfavourable, he would not rest until he had found such a demonstration as would be convincing to all.'

Galileo's efforts to secure employment in Bologna were frustrated, and he was equally unfortunate in successive applications for similar posts at Padua, Pisa, and Florence. Disappointed by repeated rebuffs, he and a young Florentine friend decided, towards the end of May, 1589, to seek their fortunes in the East. They had reached Milan, and were setting out for Venice *en route* to Constantinople, when the mathematical chair at Pisa became again vacant. Once more he applied for the post ; and this time was successful, through the joint influence of his Maecenas, the Marquis Guido Ubaldi del Monte, and Cardinal Francesco del Monte, brother of the Marquis. He was still barely twenty-five and a half years old. The salary itself was insignificant, only sixty scudi per annum, or about £14 of our money ; moreover, the appointment was for three years only, though renewable. But in his needy circumstances even this meagre opportunity was not to

be rejected, and the office would after all enable him to add something to his means by private tuition.

### 6. *Certain Mathematical Problems*

No sooner was he settled in his new office than he resumed his physico-mathematical investigations. In the first year he carried to greater length his studies on the centre of gravity of solids, and arrived at results which excited afresh the admiration of Guido Ubaldi del Monte. At about the same time he undertook the investigation of that particular curve which is described by a point in a circle (as a nail in the rim of a wheel) while it revolves along a surface. To this curve thus described he gave the name Cycloid. This curve (known as Aristotle's wheel) and its properties had long been a puzzle to geometers, and had passed into a proverb —' Rota Aristotelis quae magis torquet quo magis torquetur.' He attempted the problem of its quadrature, and guessed that the area contained between the cycloid and its base is three times that of the describing circle, but he was unable to show this geometrically —a task which his disciple Torricelli (1608–47) achieved soon after his death.[1] Galileo recommended the curve as a form of arch for bridges, and it is said to have been applied in the construction of one across the Arno at Pisa (? the present Ponte di Mezzo).

Side by side with these studies Galileo was at this time steadily revolving those novel ideas on motion which were to be the basis of his latest and greatest work. In pursuance of these ideas, he now began a systematic experimental investigation of the mechanical doctrines of Aristotle.

Galileo was not the first to call in question the authority of Aristotle in matters of science. Cardinal Nicholas de Cusa (1401–64) was among the earliest to enter the lists.[2] The philosopher, Peter Ramus (1515–72), was another early opponent, and suffered the penalty for his convictions in the massacre of St. Bartholomew. Leonardo da Vinci (1452–1519) also held some very correct views on mechanics, and even anticipated Galileo in a few instances.[3]

[1] *Opera Geometrica*, Florence, 1644. Cf. Nat. Ed., vol. xviii, p. 153. Huygens applied the tautochronic property of this curve to the better regulation of pendulum clocks.

[2] Cusa, *De Docta Ignorantia*, Paris, 1514.

[3] His writings, mostly short notes and memoranda, were not known in Galileo's time. They remained in MS., practically lost to the world, till 1797, when Venturi brought them to light in his *Essai sur les Ouvrages physico-mathématiques de Léonard de Vinci*, Paris, 1797. Cf. Favaro's ' Leonardo da Vinci e Galileo ' (*Estratto dalla Raccolta Vinciana*, July 1906), and ' Léonard de Vinci a-t-il exercé une influence sur Galilée et son École ? ' (*Scientia*, December 1916).

Rizzoli,[1] again, in a posthumous work, had condemned the peripatetic philosophy in forcible terms, declaring that, although there were many excellent truths in Aristotle's *Physics*, the number was scarcely less of false, useless, and ridiculous propositions. Giovanni Battista Benedetti,[2] another sixteenth-century writer, had written expressly to confute several of Aristotle's mechanical problems, and so clearly expounded some principles of statical equilibrium that he may be regarded as a precursor of Galileo. Tartaglia [3] had discussed the theory of projectiles, and Varrone wrote on the force of inertia, and recognized the power of gravity as the cause of accelerated motion in falling bodies.

But while Galileo was thus by no means the first to question the authority of Aristotle, he was undoubtedly the first whose questioning produced a profound and lasting effect in men's minds. The reason is not far to seek. Galileo came at the fitting moment, but, above all, he came armed with a new instrument—*experiment*.

### 7. *Sermones de Motu Gravium* [4]

The results of these earlier isolated investigations on the foundations of dynamics are given at great length in the treatise *Sermones de Motu Gravium*, written in 1590, and, as was then the custom of Galileo and for many years after, the work was first circulated in manuscript. It did not appear in print until two hundred years after his death,[5] and then in an incomplete form. These *Sermones* consist chiefly of objections to Aristotelian doctrines, but a few of the chapters are devoted to an entirely new field of speculation. Thus, the 11th, 13th, and 17th *Sermones* relate to the motion of bodies along planes inclined at various angles, and of projectile motion ; the 14th enunciates a new theory of accelerated motion ; while in the 16th his assertion, that a body falling naturally for however great a time would never acquire more than an assignable degree of velocity, shows that he had already formed accurate notions of the action of a resisting medium.[6]

[1] Rizzoli, *Antibarbarus philosophica*, Frankfurt, 1674.

[2] Benedetti, *Speculationum liber*, Venice, 1585.

[3] Tartaglia, *Quesiti et Inventioni diverse*, Venezia, 1546.

[4] Cf. Nat. Ed., vol. i, pp. 245–419.

[5] In Alberi's *Le Opere di Galileo*, Florence, 1842–56.

[6] Most of these theorems were afterwards developed and incorporated in his larger work, *Dialoghi delle Nuove Scienze*, Leyden, 1638, and they can best be studied in that admirable compendium.

Galileo did not content himself with writing and circulating his *Sermones*, but as soon as he succeeded in demonstrating the falsehood of any Aristotelian proposition he did not hesitate to denounce it from his professorial chair.

8. *Public Experiments on Falling Bodies*, 1590–1 [1]

From professorial denunciation he proceeded to those public experiments with which the leaning tower of Pisa has become for ever associated. Aristotle had said that, if two different weights of the same material were let fall from the same height, the two would reach the ground in a period of time inversely proportional to their weights. Galileo maintained that, save for an inconsiderable difference due to the disproportionate resistance of the air, they would fall in the same time. The Aristotelians ridiculed such ' blasphemy ', but Galileo determined to make his adversaries see the fact with their own eyes. One morning, before the assembled professors and students, he ascended the leaning tower, taking with him a 10 lb. shot and a 1 lb. shot. Balancing them on the overhanging edge, he let them go together. Together they fell, and together they struck the ground.

Neglecting the resistance of the air, he now boldly announced the law that all bodies fall from the same height in equal times. The correctness of this law was easily, if roughly, established by the leaning tower experiments, but, as the vertical fall was too rapid to admit of exact measurement, he made use of the inclined plane —a long straight piece of wood, along which a groove was accurately made, and down which bronze balls were free to move with the least friction. With this he proved that, no matter what the inclination of the plane, and, consequently, no matter what the time of fall, the movement of the balls was always in accordance with his law. In these investigations he utilized his discovery of the isochronism of pendulum oscillations as a measurement of time.

It might have been thought that such experiments would have settled the question. Yet with the sound of the simultaneously fallen weights ringing in their ears, the Aristotelians still maintained that a weight of 10 lb. would reach the ground in a tenth of the time taken by a weight of 1 lb. A temper of mind like this could not fail to produce ill-will, and we learn that, with the exception of the newly appointed Professor of Philosophy,

[1] Cf. Nat. Ed., vol. i, p. 249 ; vol. xix, p. 606.

Jacopo Mazzoni, the whole body of the teaching staff now turned against their young colleague.

> Stimano infamia il confessar da vecchi
> Per falso quel che giovani apprendero.
> Viviani, after *Horace*.

Soon a wholly unforeseen circumstance came to their aid, and led to Galileo's retirement from Pisa. Giovanni de Medici, natural son of Cosimo I, was at the time Governor of Leghorn. He was not unskilled as an engineer and architect, and had himself just designed a monster machine which he wished to use in cleaning the harbour. A model was submitted to the Grand Duke, and Galileo commissioned to examine and report on it. He did so, and declared it useless—an opinion which subsequent trial fully confirmed. Smarting under this failure, the inventor was induced to combine with the Aristotelians, to whose machinations were now added intrigues at Court. The position became intolerable, and Galileo resigned his post before the three years' term had expired, and once more returned to Florence about the middle of 1592.

## III. Life Work, 1593–1632

### 1. *Early Years at Padua*

Galileo had not long to wait for a new post, for on the 22nd of September, 1592, he was appointed to the mathematical chair in Padua. Here he displayed at once extraordinary and varied activity. Besides the routine lectures on Euclid, the Sphere, and Ptolemy's Almagest, he gave special courses on Military Architecture and Fortifications,[1] on Mechanics, and on Gnomonics. On these and other subjects he prepared treatises which long circulated in manuscript among his pupils. Some were printed many years afterwards; others, like the treatise on Gnomonics, are lost; while others again found their way into the hands of persons who did not scruple to claim and publish them as their own.

The treatise on the Sphere, first published in Rome in 1656,[2] is supposed by some to be apocryphal, as it teaches the Ptolemaic cosmogony, placing the earth immovable in the centre of the

[1] Cf. Nat. Ed., vol. ii, pp. 9–146. In this there is nothing very original, his object being to lay before the student a compendium of the most approved principles of military science as then known.

[2] *Trattato della Sfera di Galileo*, &c., by Urbano Daviso, under the pseudonym of Buonardo Savi. Cf. Nat. Ed., vol. ii, pp. 205–55.

universe, and adducing the usual orthodox arguments. But we have it from Galileo's own hand that for many years he taught the Ptolemaic system in his classes in deference to popular feeling, although at heart he was even then a follower of Copernicus.[1]

### 2. *On Mechanics* [2]

The treatise *Delle Meccaniche* was written in 1594, and deals with the powers of the balance, the lever, the windlass, the screw, the pulley, cogged wheels, and the endless screw ; and concludes with a note on ' The Force of Percussion ', added later, apparently in 1599. Galileo had not yet discovered the principle of the decomposition of forces, but in his present treatment of these subjects he ingeniously uses the theory of the screw, reducing the screw to the inclined plane, the inclined plane to the pulley, and the pulley to the simple lever. Here for the first time is mentioned that condition of equilibrium, regarded from an entirely new aspect, to which modern mechanics owes all its splendid achievements. This condition of equilibrium is now called the principle of virtual velocities, and the credit of its discovery is with Galileo, who undoubtedly perceived its importance.

### 3. *Machine for Raising Water* [3]

While carrying on his professorial duties, giving private lessons, and writing learned tracts, Galileo was occupied in giving practical form to his ideas on the proper application of mechanics to machinery. Thus, towards the end of 1593, he devised a machine for raising water, of small dimensions but of great power, so constructed that one horse could raise the water and distribute it through twenty channels. The Venetian Government gave him the monopoly of this invention for a period of twenty years, though it does not appear to have been used in a practical way.[4]

[1] See his letter to Kepler dated August 4, 1597. His lectures on the New Star (p. 220, *infra*) may be regarded as his first *public* note of antagonism to the ruling astronomy. Cf. Favaro, *Galileo e lo Studio di Padova*, vol. i, pp. 148–67.

[2] Cf. Nat. Ed., vol. ii, p. 149 ; vol. viii, pp. 216, 321.

[3] Cf. Nat. Ed., vol. xvi, p. 27 ; vol. xix, p. 126.

[4] The early biographers of Galileo, Viviani and Gherardini, state that he was often employed ' to his great honour and profit ' in the construction or superintendence of other machines for use in the Venetian State, but repeated searches in the archives of Venice and Padua do not afford any ground for this statement. Cf. Favaro, *Intorno ai servigi straordinarii prestati da Galileo alla Republica Veneta*, Venezia, 1890.

### 4. *The Geometrical and Military Compass* [1]

In 1597, however, he invented a more profitable instrument, and one that came into very extensive use, the Geometrical and Military Compass. This instrument is now known as *Gunter's Scale*, or the *Sector*. It consists of two straight rulers connected by a joint so that they can be set to any required angle. On one side are four sets of lines :

*Arithmetical lines,* which serve for the division of lines, the solution of the Rule of Three, the equalization of money, the calculation of interest.

*Geometrical lines,* for reducing proportionally superficial figures, extracting the square root, regulating the front and flank formations of armies, and finding the mean proportional.

*Stereometrical lines,* for the proportional reduction of similar solids, the extraction of the cube root, the finding of two mean proportionals, and for the transformation of a parallelopiped into a cube.

*Metallic lines,* for finding the proportional weights of metals and other substances, for transforming a given body into one of another material and of a given weight.

On the other side of the instrument are :

*Polygraphic lines,* for describing regular polygons, and dividing the circumference of the circle into equal parts.

*Tetragonical lines,* for squaring the circle (approximately) or other regular figure, for reducing several regular figures to one figure, and for transforming an irregular rectilineal figure into a regular one.

*Joined lines,* used in the squaring of the various portions of the circle and of other figures contained by parts of the circumference, or by straight and curved lines together.

There is joined to the compass a quadrant, which, besides the usual divisions of the astronomical compass, has transversal lines for taking the inclination of a scarp of a wall.[2]

### 5. *The Thermometer* [3]

To a somewhat later period may be referred his invention of the air thermometer. The date is uncertain, for while Viviani asserts that the instrument was designed during the first term of

[1] Cf. Nat. Ed., vol. ii. pp. 337–601 ; vol. xix, pp. 167, 222.

[2] The treatise on this subject, *Le Operazioni del Compasso geometrico e militare* (1606), is Galileo's first printed work, and was entirely set up in his own house in Padua.

[3] Cf. Nat. Ed., vol. xvii, p. 377.

his professorship at Padua (1592–8), other evidence takes us back only to about 1602. Thus Benedetto Castelli (1577–1643), writing to Ferdinando Cesarini, on the 20th of September, 1638, says:

'I remember an experiment which our Signor Galileo showed me more than thirty-five years ago. He took a small glass bottle about the size of a hen's egg, the neck of which was about two palms long [about 22 inches], and as narrow as a straw. Having well heated the bulb in his hand, he inserted its mouth in a vessel containing a little water, and, withdrawing the heat of his hand from the bulb, instantly the water rose in the neck more than a palm above its level in the vessel. It is thus that he constructed an instrument for measuring the degrees of heat and cold.'

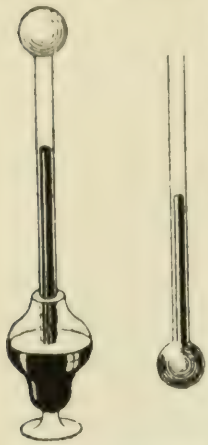


Fig. 5. Galileo's thermometer.

In this instrument different degrees of temperature were indicated by the expansion or contraction of the air which remained in the bulb; so that the scale was the reverse of that of the thermometer now in use, for the water would stand at the highest level when the weather was coldest. So long as the orifice of the tube remained open, this instrument could not be an efficient measurer of temperature, for it would be impossible to distinguish the effects of heat and cold from the effects of varying atmospheric pressure. It was, in truth, a barometer as well as thermometer, although Galileo did not recognize this (Fig. 5).

His friend Sagredo of Venice (1571–1620) was the first to divide the tube into 100 degrees in 1613. Sagredo also appears to have experimented with closed tubes from about 1615; but it was not until many years later (1653), after the death of Galileo, that the practice of closing the orifice, after exhausting the air, was introduced.[1]

## 6. *New Star of* 1604 [2]

In 1604 a new star appeared with great splendour in the constellation Serpentarius. Maestlin, one of the first to notice it, says:

'How wonderful is this new star! I am certain that I did not

[1] The credit of this capital improvement is due to Leopoldo de' Medici, brother of Ferdinando II, who adopted the plan of filling the tube with spirit, boiling it, and sealing the orifice whilst the contained spirit was in an expanded state, thus obtaining a partial vacuum, and depriving the instrument of its barometrical property.

[2] Cf. Nat. Ed., vol. ii, pp. 269–305, 526.

see it before the 29th of September, nor, indeed, on account of several cloudy nights had I a good view till the 6th of October. Now that it is on the other side of the sun, instead of surpassing Jupiter as it did, and almost rivalling Venus, it scarcely equals the Cor Leonis, and hardly surpasses Saturn. It continues, however, to shine with the same bright and strongly sparkling light, and changes colour almost every moment, now tawny, then yellow, presently purple and red, and, when it has risen above the vapours, most frequently white.'

Galileo appears to have noticed the new star about the 15th of October. The appearance of the new phenomenon had given rise to the most bewildering statements. Some said it was a light in the inferior regions of space, in 'the elementary sphere', that is, in that sphere of the four elements below that heavenly and incorruptible region where the Aristotelian school placed the heavenly bodies. Others thought that it was an old star hitherto unnoticed; others again believed that it was a new creation, while the astrologers deduced from it the wildest forebodings.

After observing the new star for some time, Galileo expounded his views upon it in three extraordinary lectures, which were delivered to the public in the great hall of the University in December, 1604. Unfortunately, only fragments of these lectures have come down to us, but from these and one or two other references,[1] we learn that his purpose was to indicate the position of the new phenomenon as 'far above the sphere of the Moon'.

Now, unlike his contemporaries, Tycho Brahé and Kepler, who thought that new stars were temporary conglomerations of a vapour-filling space, Galileo had suggested that they might be products of terrestrial exhalations of extreme tenuity, at immense distances from the earth, and reflecting the sun's rays—an hypothesis which, as we shall see later on, he also applied to comets. From the absence of parallax he showed that the new star could not be, as the current theory held, a meteor engendered in our atmosphere, and nearer to us than the moon, but that it must be situated beyond the planets and among the remote heavenly bodies. This was inconceivable to the Aristotelians, who

[1] Cf. *Difesa contro alle Calunnie di Baldassare Capra*, Nat. Ed., vol. ii, and *Postille al Libro d'Antonio Rocco*, Nat. Ed., vol. vii.

thought of the outer sphere of the Universe as an incorruptible and unchangeable heaven, subject neither to growth nor to decay, where nothing was created or destroyed.

### 7. *On Magnetism* [1]

Galileo's study of the magnetic properties of the loadstone dates from about 1600, apparently after he had seen the *De Magnete* of William Gilbert of Colchester (1544–1600), which was published in London in 1600. Gilbert's book had very great attraction for him, firstly, because its arguments traversed many of the principles of the Aristotelian school, and secondly, because it contained a number of original experiments, coupled with philosophical reflections of a far-reaching kind which appealed to his own daring spirit. Both Sagredo and Paolo Sarpi (1552–1623), his Venetian friends, tell us that at this time (1602) Galileo had not only repeated many of Gilbert's experiments, but made new ones of his own, especially on the best method of arming loadstones.

During the long vacations at Padua Galileo was wont to return to Florence, where he gave lessons to the young Prince Cosimo de' Medici. In the summer of 1607 he had evidently been rehearsing the wonderful properties of loadstones, and on returning to Padua he sent his pupil a stone which he had picked up in Venice, about ½ lb. in weight, not elegant in form but very powerful. In the letter which accompanied it he speaks of another stone belonging to Sagredo.

'This is elegantly shaped, and weighs about 5 lb. I have made it capable of sustaining 5½ lb. of iron, and I think I can make it do more before it leaves my hands. Much diligence is required in finding the true poles of these stones, that is, where their virtue is most robust, and where, consequently, their full sustaining power is manifested. This sustaining force depends as much on the quality of the armature as on the stone itself. Not every piece of iron of any size and shape is equally sustained, but well-made steel of a particular size and figure is most powerfully attracted. Then again, a slight shifting of the armatures from their true positions causes a great variation in the sustaining force. In the last four days I have so arranged that the stone now bears 1 lb. more than its owner was ever able to make it carry, and I hope, after preparing some pieces of the finest steel, to make it sustain still more.'

[1] Cf. Nat. Ed., vol. iii, p. 279 et seq.; vol. x, pp. 89, 185 et seq.; vol. xiii, p. 328.

He then describes a curious case of what is now called superposed magnetism :

'I have also observed in this stone another admirable effect which I have not met with in any other, namely, that the same pole repels or attracts the same piece of iron according to distance. Thus, placing an iron ball on a smooth and level table, and quickly presenting the stone, at a distance of about one finger, the ball moves away, and can be chased about at pleasure. But now, when the stone is sharply withdrawn to a distance of about four fingers, the ball moves *towards* it, and, with a little dexterity, can be made to follow it about.'

Ultimately Galileo was able to secure this stone also for Prince Cosimo, and in an accompanying letter to Chief Secretary Belisario Vinta, dated 3rd May, 1608, he writes :

'I send for his Highness the loadstone which, after many experiments, I have finally made to sustain a weight of 12 lb., or more than double its own, and I am certain that, had I more time and more suitable tools at my disposal, I could have done better still. I have fashioned the two armatures in the form of anchors, in allusion to the fable of a stone so large and powerful as to hold securely a ship's anchor. The form is also convenient for attaching weights to measure its holding capacity. I have purposely not made these anchors equal to the largest weights that I know the stone will carry, first, to leave a margin of safety, so that it will hold them firmly when presented, and secondly, because I am of opinion that its power may vary according to locality with respect to the poles of the great magnet, the earth ; for, whereas along the equinoctial line both poles will be of equal strength, one may be more powerful than the other in the northern hemisphere, and vice versa in the southern. Hence I am led to believe that the more powerful pole sustains here in Padua somewhat more than it can in the more southerly latitude of Florence or Pisa.[1] . . .

'I have marked the poles, so that one can readily see where the anchors or armatures should be applied ; that with the greater weight should be attached to the more robust pole, and the less heavy one to the weaker pole, taking care to apply them at the same instant, for I have found, not without great astonishment, that the stone more willingly supports two weights together than one alone. Thus, a piece of iron, so heavy as not to be supported when applied alone, will be held if at the same time another piece is applied to the other pole. . . . I have in mind some other

[1] Here he confuses two properties of the magnet, (1) its portative force, and (2) its directive force or 'inclination'.

artifices to render the stone still more marvellously powerful, and I am certain that I shall not fail. I believe I can make it sustain four times its own weight, or 20 lb., which, for such a large stone, is something very admirable. Indeed, I have no doubt that with proper cutting it can be made to support more than 30, and even 40 lb. I have noticed in this stone, not only that it never tires of holding a weight, but that with time it invigorates itself the more.' [1]

The 'other artifices' referred to in the letter consisted in breaking up large stones, shaping the best pieces so as to bring out their maximum of polarity, and providing them with suitable armatures, with the result that the portative force of the selected pieces far exceeded his own first and Gilbert's achievements in the same direction. Thus, in the *De Magnete*,[2] the English philosopher speaks of a stone (weight not given) which normally could sustain 4 oz. of iron, but which, when capped with steel, could support 12 oz. 'But', he goes on, 'the greatest force of a combining, or rather united, nature is seen when two stones, armed with iron caps, are so joined by their concurrent (commonly called contrary) ends that they mutually attract and raise one another. In this way a weight of 20 oz. of iron is raised, when either stone unarmed would only allure 4 oz.'

Galileo went far beyond this, since he was able to fashion small stones of extraordinary power. Of such a one he speaks in a letter to Cesare Marsili, dated 27th June, 1626. It weighed 6 oz., and unarmed could only support 2 oz., but when armed it was able to sustain 160 oz., or twenty-six times its own weight. He had this by him when writing his Dialogue of 1632, where he speaks of it as still in his possession. Later he appears to have presented it to the Grand Duke of Tuscany, Ferdinando II, as we gather from Castelli's *Discorso sopra la Calamita* (*circa* 1639–40).[3]

[1] These two stones were lost in after years, for in 1698 Leibnitz searched for them in vain.

[2] Chapter XVII, book 11.

[3] This stone is now preserved in the Tribuna di Galileo, Florence. The weight is in the form of a tomb (*sepolcro*), a form which was probably suggested by the legend of Mohammed's coffin suspended in the air by loadstones.

The Editor of the Florentine edition of Galileo's Works, 1718, mentions another small stone fashioned by Galileo, and of still more extraordinary power. It weighed only three-tenths of a grain, yet could support 121 grains (vol. i, Preface, p. xlvi). This, if true, beats Sir Isaac Newton's stone, which he wore set in a ring. It weighed 3 grains, and was able to support 746 grains.

8. *Development of Galileo's Ideas on Mechanics*, 1590–1609 [1]

In the years 1590 to 1609, Galileo completed, or, at least, laid the foundations of, his *Dialoghi delle Nuove Scienze* of 1638. In the 6th Day of these dialogues, he speaks of Paolo Aproino as assisting at a great number of experiments in Padua on diverse problems in mechanics, and especially, ' on the marvellous problems of percussion '. On the 29th of November, 1602, we find him writing to the Marquis Guido Ubaldi on the fall of bodies through two successive chords of a quadrant (discussed in 3rd Day), and on pendulum oscillations of different amplitudes. In 1604–6 he further studied the properties of the inclined plane, and took up the subject of naturally accelerated motion (discussed in 3rd Day). During 1609 he was occupied with the strength of beams and their resistance to fracture (discussed in 2nd Day). In the same year he investigated the motion of projectiles as applied to artillery, using the experiences derived from his many visits to the arsenal in Venice (discussed in 4th Day), and the coherence of the particles of solid bodies (discussed in 1st Day). In fact, about May, 1609, he was intending to publish an account of all these studies as a complete system of mechanical philosophy, when the telescope and consequent astronomical work intervened, and turned his energy in another direction.

Writing of this period, 1602–9, Favaro says:

' In truth the house of Galileo in Padua was not only a place for genial intercourse ; not only a school to which flocked students, Italians and foreigners from every country in Europe, but more than this, it was a laboratory, where his marvellous mechanical talent knew how to devise ever new expedients. It was an academy in the true sense of the word, where the gravest problems in physics, in mechanics, in astronomy, and in mathematics were discussed with perfect freedom, and where it was possible to submit the deductions of reason to the salutary test of experiment, and the results of experiments, in their turn, to reasoning and calculation. Thus it may be said that the principal problems in the *Dialoghi delle Nuove Scienze* were raised and discussed within the walls of Padua.' [2]

[1] Cf. Nat. Ed., vol. ii, p. 259 ; vol. x, pp. 97, 228, 244, 248.

[2] *Galileo e lo Studio di Padova*, 2 vols., Firenze, 1883, vol. i, p. 314. Much the same may be said of his Dialogue of 1632, where many of the arguments are drawn from his experiments in Padua.

Galileo had long been desirous to return to his native Tuscany, and he now (1609) opened negotiations with the Chief Secretary of the Grand Duke, explaining that what he especially sought was leisure to pursue his researches without the distraction and strain of teaching and lecturing.

'The works which I have to finish', he wrote in May 1610, 'are chiefly: (1) two books on the system or structure of the Universe, an immense undertaking full of philosophy, astronomy, and geometry; (2) three books on local motion, a science entirely new, no one, ancient or modern, having discovered any of the many admirable consequences which I demonstrate in natural and violent motions, so that I may with reason call it a new science invented by me from its very first principles; (3) three books on mechanics, two of them on the demonstration of principles, and one of problems. Although others have treated this subject, no one either in quantity or quality has done a quarter of what I am writing on it. I have also treatises on natural subjects such as on sound and speech; on light and colours; on the tides; on the composition of continuous quantity; on the movements of animals; and others. I have also an idea of writing some books relating to the military art. . . .

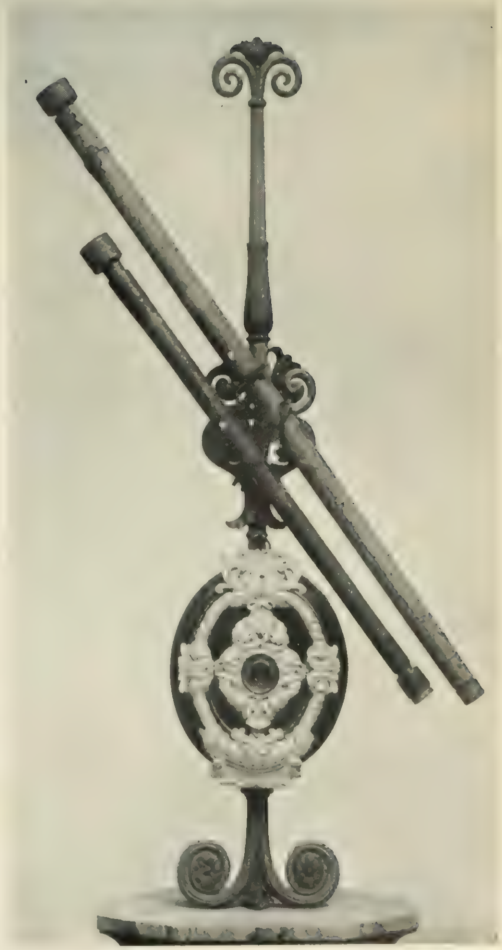
'Then I need not say what an amount of labour will be required to fix the periods of the four new planets, a task the more laborious the more one thinks of it, as they are separated from one another by very brief intervals, and are all very similar in size and colour.'

Some of the treatises named in this letter are now lost, partly through the accidents of Galileo's stormy life, and in transport from place to place, and partly, through the extraordinary negligence and criminality of custodians. The loss of the essay on Continuous Quantity is particularly to be regretted. It is to his early disciple Buonaventura Cavalieri (1598–1647), who refused to publish his own book [1] so long as he hoped to see Galileo's printed, that we owe *The Method of Indivisibles*, which is recognized as containing some of the germinal ideas of Newton's Fluxional Calculus. The treatises on *Sound and Speech*, and on *Light and Colours*, were probably never completed, but we find fragments of them in later works, as, *Il Saggiatore* and the *Dialogues* of 1632 and 1638. Similarly, of the movements of animals we have the fragment *Intorno al camminare del cavallo*.[2]

[1] *Geometria indivisibilibus continuorum nova quadam ratione promota*, Bologna, 1635.

[2] Nat. Ed., vol. viii, last section.

Galileo's Lodestone and Military Compass



Galileo's Telescopes
The cracked lens is mounted in centre

### 9. *The Invention of the Telescope*[1]

Leaving aside the vexed question of the priority of invention of the telescope, and of the reports of it which circulated in Italy from about November 1608,[2] we pass to Galileo's own account of his share in the discovery. Of this there are three slightly discordant versions—one to his brother-in-law, Landucci, dated 29th August 1609, the second in *Sidereus Nuncius*, published in March 1610, and the third in *Il Saggiatore*, 1623. In the first version (probably the most reliable, as being nearest in time) he says:

'I write now because I have a piece of news for you, though whether you will be glad or sorry to hear it I cannot say, for I have now no hope of returning to my own country, though the occurrence which has destroyed that hope has had results both useful and honourable. You must know then that about two months ago (i. e. about June 1609) a report was spread here that in Flanders a spy-glass had been presented to Prince Maurice, so ingeniously constructed that it made the most distant objects appear near, so that a man could be seen quite plainly at a distance of two miles. This result seemed to me so extraordinary that it set me thinking, and, as it appeared to me that it depended upon the laws of perspective, I reflected on the manner of constructing it, and was at length so entirely successful that I made a spy-glass which far surpasses the report of the Flanders one. As the news had reached Venice that I had made such an instrument, six days ago I was summoned before their Highnesses the Signoria, and exhibited it to them, to the astonishment of the whole Senate. Many of the nobles and senators, although of a great age, mounted more than once to the top of the highest church tower in Venice, in order to see sails and shipping that were so far off that it was two hours before they were seen without my spy-glass, steering full sail into the harbour; for the effect of my instrument is such that it makes an object 50 miles off appear as large as if it were only five miles away.

'Perceiving of what great utility such an instrument would prove in naval and military operations, and seeing that his Serenity the Doge desired to possess it, I resolved on the 24th inst. to go to the palace and present it as a free gift. On quitting the presence-

[1] Cf. Nat. Ed., vol. iii, pp. 18, 60, 869; vol. x, pp. 250–3; vol. xix, p. 587.

[2] Favaro discusses these questions very fully in *Galileo e lo Studio di Padova*, vol. i, ch. xi, and in *La Invenzione del Telescopio secondo gli ultimi Studi*, Venezia, 1906. Cf. Drinkwater's *Life of Galileo*, 1833, pp. 20–6; Grant's *History of Physical Astronomy*, 1852, pp. 514–37.

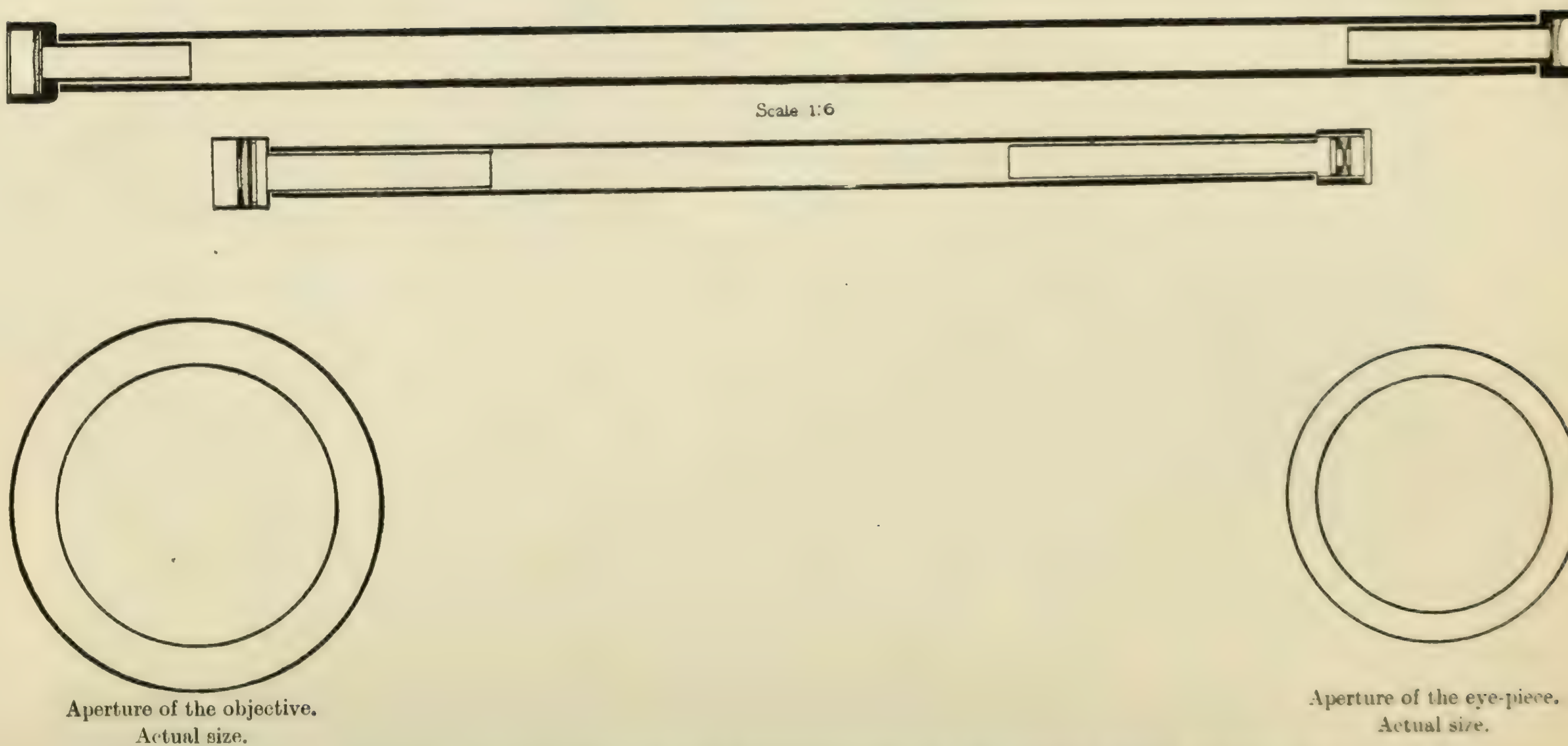



Fig. 6. DIAGRAM TO ILLUSTRATE THE PROPORTIONS OF GALILEO'S FIRST TELESCOPES

chamber I was commanded to bide awhile in the hall of the Senate, whereunto the Procurator, Antonio Prioli, one of the heads of the University of Padua, came, and, taking me by the hand, said that the Senate, knowing the way in which I had served it for seventeen years at Padua, and being sensible of my courtesy in making it a present of the spy-glass, had ordered my election (with my goodwill) to the Professorship for life.'

This telescope is unfortunately lost. It is mentioned with some particulars in his *Sidereus Nuncius*, and consisted of a leaden tube, with a plano-concave eye-glass and a plano-convex object-glass, and had a magnifying power of three diameters. From other sources we learn that the tube was 2·39 metres long and about 42 millimetres diameter (see Fig. 6, p. 228). This instrument was shown for the first time in public on the 21st of August, 1609, from the top of the campanile of San Marco, when the farthest object that could be seen clearly was the campanile of the Church of San. Giustina in Padua, distant about 35 kilometres.

### 10. *Invention of the Microscope* [1]

The invention of the telescope could hardly fail to lead to the disclosure of the principle of the microscope, which at first was but a telescope adapted. John Wodderborn, a Scotch student who attended Galileo's lectures at Padua, in a defence of his master (published in 1610),[2] states that he ' heard Galileo describe in what manner he perfectly distinguishes with his telescope the organs of motion and of the senses of the smaller animals, especially in a certain insect which has each eye covered by a rather thick membrane, which, perforated with seven holes, like the visor of a warrior, allows it sight '.[3]

In 1614, the Frenchman, Jean Tarde, called on Galileo, whom he found ill in bed, and he says that

' Galileo told me that the tube of a telescope for observing the stars is no more than 2 feet in length ; but to see objects well, which are very near, and which on account of their small size are hardly visible to the naked eye, the tube must be two or three times longer. He tells me that with this long tube he has seen flies which look as big as a lamb, are covered all over with hair,

[1] Cf. Nat. Ed., vol. xiii, pp. 36, 40, 199, 201, 208.

[2] John Wodderborn, Scotobritannus, *Quatuor problematum quae Martinus Horky contra Nuntium Sidereum de quatuor planetis novis disputanda proposuit confutatio*, Padua, 1610.

[3] Nat. Ed., vol. iii, pp. 151–78.

and have very pointed nails, by means of which they keep themselves up and walk on glass, although hanging feet upwards.' [1]

In *Il Saggiatore* and in Viviani's biographical sketch there are similar references, the latter adding that in 1612 Galileo presented a microscope to the King of Poland. All this shows that he was well acquainted from the first with the use of his invention *qua* microscope. For many years, however, he gave the matter little attention—not, indeed, until his visit to Rome in 1624, when he found the microscope discussed as a novelty which nobody could understand.

An optician of Middelburg had invented a form of microscope about 1590, in which objects were seen inverted. One of these instruments he presented to the Archduke Charles Albert of Austria, who in turn gave it to Cornelius Drebbel, a Dutchman, then living in London. For many years after, the instrument was practically forgotten; but about 1621 Cornelius Drebbel appears to have resumed its manufacture.[2] In the following year Jacob Kuffler, a relative of Drebbel, brought a specimen to Rome, a present from Nicholas Fabri de Peiresc of Paris (1580–1637) to one of the Cardinals. Unfortunately, Kuffler died before he had time to explain the management of the instrument, and so it remained a mystery. Two years later two other specimens arrived, also sent by de Peiresc, with brief instructions as to their use. One of these was nothing more than a magnifying glass, but of the other, consisting of two glasses, nobody in Rome could make anything, 'although they had the help of mathematicians'.

At this moment Galileo arrived. The instrument was shown him, and he at once told his friends that he had himself made a somewhat similar instrument many years previously, 'which magnified things as much as 50,000 times, so that one sees a fly as large as a hen'. He made some specimens, showing objects *erect*, which he sent to his friends, and soon his microscopes were in as great request as his telescopes. Amongst others, he sent one to Prince Federigo Cesi, on the 23rd of September, 1624, with the following letter:

[1] Cf. Nat. Ed., vol. xix, p. 589. Tarde's voyages in Italy are in MS. at the Bibliothèque nationale, Paris.

[2] Charles Singer, 'Notes on the Early History of the Microscope', in the *Proceedings of the Royal Society of Medicine, Historical Section*, 1914, vol. vii, p. 247, and 'The Dawn of Microscopical Discovery,' *Journal of the Royal Microscopical Society*, 1915, p. 317, and the article by him in this volume.

'I send your Excellency a little spy-glass (*occhialino*) for observing at close quarters the smallest objects, which I hope will afford you the same interest and pleasure that it has to me. I delayed sending it because my first attempts were imperfect by reason of the difficulty in fashioning the lenses. The object is placed on a movable circle (at the base of the instrument), which can be turned in such a way as to show successive portions, a single pose being unable to show more than a small part of the whole. As the distance between the lens and the object must be precisely adjusted in order to see things that are in relief, it is necessary to bring the glass nearer to or farther from the object according to the parts to be examined. Therefore the little tube is made adjustable on its stand or guide. The instrument should be used in a strong light, or even in full sunlight, so as to illuminate the object as much as possible.

'I have examined with the greatest delight a large number of animals, amongst which the bug is most horrible, the gnat and the moth very beautiful. I have also been able to discover how the fly and other little animals are able to walk on window-panes and ceilings feet upwards. But your Excellency will now have the opportunity of observing thousands of other details of the most curious kind, of which I shall be glad to have an account.

'P.S.—The little tube is in two pieces, so that you may lengthen it or shorten it at pleasure.' [1]

## 11. *The Siderius Nuncius*

### *First Telescopic Discoveries in the Heavens* [2]

After exhibiting his telescope in Venice, Galileo returned to Padua, and at once constructed a third instrument, of which he only says that 'it made objects appear more than sixty times larger', equivalent to a magnifying power of about eight diameters. But in a very few days he had a much better telescope which enlarged four hundred times. With this in the autumn of 1609 he made his first discoveries in the heavens—an immense number of fixed stars, more than tenfold the number at that time catalogued. He noticed the property of irradiation common to all

[1] The only relics (two tubes) of these instruments now known to exist are preserved in the Tribuna di Galileo, Florence. The lenses are missing, and the genuineness of the tubes themselves is doubtful. For much interesting information on this subject see Prof. Govi's 'The Compound Microscope invented by Galileo', in *Journal of the Royal Microscopical Society*, 1889, pp. 574 et seq., and the two articles by Charles Singer quoted above.

[2] Cf. Nat. Ed., vol. iii, Part 1, *passim*; vol. x, pp. 273, 410; vol. xix, pp. 229, 610.

luminous bodies, and the scintillation of the fixed stars, which differentiated them from the planets. Upon directing the telescope to the more conspicuous star-clusters, he was astonished to find that they contained a great number of other stars too faint to be recognized by the naked eye. Thus, the number of the Pleiades, from six or seven, now rose to thirty-six ; while in Orion, instead of thirty-seven, he now counted as many as eighty stars. Next, examining portions of the Milky Way and other nebulous patches he resolved them into congeries of stars of various magnitudes. Turning to the moon, he brought it to a distance of less than three semi-diameters of the earth, making it appear about twenty times nearer and four hundred times larger than when seen by the unaided eye.

Early in January, 1610, Galileo had constructed a still more powerful telescope, which showed objects more than thirty times nearer and nearly one thousand times larger. With this instrument he not only verified and completed the observations begun the previous autumn with his fourth telescope, but he also discovered Jupiter's moons.

Writing to a friend at the Tuscan Court on 30th January, 1610, he thus alludes to this series of discoveries :

'I give thanks to God, who has been pleased to make me the first observer of marvellous things unrevealed to bygone ages. I had already ascertained that the moon was a body very similar to the earth, and had shown our Serene Master, the Grand Duke, as much, but imperfectly, not then having such an excellent spy-glass as I now possess, which, besides showing the moon most clearly, has revealed to me a multitude of fixed stars never before seen, being more than ten times the number of those that can be seen by the unaided eye. Moreover, I have ascertained what has always been a matter of controversy among philosophers, namely, the nature of the Milky Way. But the greatest marvel of all is the discovery of four new planets. I have observed their motions proper to themselves and in relation to each other, and wherein they differ from the motions of the other planets. These new bodies move round another very great star, in the same way as Mercury and Venus, and, peradventure, the other known planets, move round the sun. As soon as my tract is printed, . . . I shall send a copy to his Highness, the Grand Duke.'

This tract is the *Sidereus Nuncius*, the preface of which is dated 4th March, 1610. In this epoch-marking treatise he gives the results of his observations. He speaks first of the moon. The discovery of spots, added to those already visible to the naked eye, and observations on the changes of light on those

spots, led him to the conclusion that the surface of the moon, far from being smooth and polished according to the then accepted opinion, was rough with deep depressions and high mountains. The brilliant parts he inferred were land, while those which remained obscure—the permanent spots—he regarded as water. The illuminated edges of the moon showed themselves smooth and without those indentations which one would expect from the inequalities of the surface. Galileo explained this appearance (1) by supposing that the mountainous parts masked each other as it were, so that at the distance of the earth the intervening depressions were not discernible, and (2) by the existence of a lunar atmosphere of a density such as to reflect the solar rays while not obstructing the vision. From the appearance of illuminated mountain-tops in the dark part of the moon, at some little distance from the broken line along which sunrise or sunset was general, he was able to judge of the height of some of the mountains, and his calculation agrees very well with the modern estimate. The higher mountains were found to rise four or five miles above the general level.

Galileo remarked the feeble light,[1] which, in the first and last quarters of the moon, makes visible to us that part of its disk which is no longer illuminated directly by the sun. After showing that the light did not originate in the moon itself, that it was not caused by sunlight passing through its body, that it was not reflected there from Venus, he concludes that it can only be due to the sunlight reflected from the earth to the moon, and thence reflected back to our eyes. He contended therefore that our earth shines, like the moon and planets, by light from the sun ; that it far exceeds the moon in luminosity ; and that since it is a moving planet it is thus fully comparable to the other heavenly bodies.

In using the telescope to examine the fixed stars and comparing them with the planets, Galileo observed a remarkable difference. While the planets showed themselves as disks, like little moons, the stars appeared but little larger than with the naked eye, just bright specks sending forth twinkling rays. He explained this apparent failure of the telescope to enlarge in proportion to its magnifying power as due to the effect of irradiation. In virtue

[1] Now known as *earthshine*. Previously noticed by Pythagoras (*c*. 580–504 B.C.), by Plato (428–347 B.C.), and by Leonardo da Vinci (1452–1519). In 1640 Fortunio Liceti held that the moon is a phosphorescent body like the Bologna Stone. This drew from Galileo a reply—his last great effort. See Nat. Ed., vol. viii, pp. 467–556.

of this principle, a light projected upon a dark ground is dilated in all directions, so as to appear larger than it really is, and the greater its luminosity the greater is the irradiation, and the larger the light appears to be. A star, then, as seen by the naked eye is a luminous point plus its dilated corona. Now, the telescope has the property of cancelling or masking, more or less according to the degree of luminosity, this false light, so that what we see is the resultant of two opposite effects, (1) the star bereft more or less of its corona, and (2) the star as enlarged by the power of the glass; and, since the resultant enlargement is but little, we must conclude, not that the telescope fails in such cases, but that the corona, though diminished, is still able to nearly counterbalance the telescopic enlargement, with the result that the apparent size is little larger than that exhibited to the naked eye.[1]

When Galileo turned his fourth telescope to the planets he saw them as little moons. Jupiter's disk was of considerable size, but in no other way did he differ from the other planets. Now, on the 7th of January, 1610, directing his fifth and more powerful glass towards Jupiter, his attention was drawn to three small but bright stars in his vicinity, two on the east side and one on the west. He at first imagined them to be fixed stars, and yet there was something in their appearance which he thought curious, and they were all disposed in a right line parallel to the plane of the ecliptic. Happening, by mere accident, as he says, to examine Jupiter again on the next night, he was surprised to find these stars now arranged quite differently. They were all on the west side, nearer to each other than on the previous evening, and at equal distances apart. He therefore waited for the following night with some anxiety, but he was disappointed, for the heavens were enveloped in clouds. On the 10th of January he could see only two stars, and they were both on the east side! He suspected that the third might be concealed behind the disk of the planet. Those visible appeared as before in the same right line, and lay in the direction of the ecliptic. Unable to account for such changes by the motion of the planet, and being fully assured that he always observed the same stars, he concluded that the motions must be referred to the stars themselves and not to the planet.

On the 11th of January he again saw only two stars, still on the east side, but the outer one was now nearly twice as large

[1] He has a great deal more on this subject in his letters to Griemberger on Lunar Mountains, in his works on Sun-spots, and in *Il Saggiatore*, which see *infra*. Irradiation was first treated as a general principle by Kepler in 1604.

as the other, although on the previous night they were almost equal. This fact, taken in connexion with the constant change of the relative positions of the stars, and the total disappearance of one of them, now revealed to him their real character. He concluded that there are in the heavens three stars revolving round Jupiter in the same way as Venus and Mercury revolve round the sun. On the 12th of January he again saw three stars, two on the east side, and one on the west. The third began to appear about three o'clock in the morning, emerging from the eastern limb of the planet; it was then very small, and discernible only with great difficulty. On the 13th of January he saw four stars, three on the west side and one on the east. They were all in a line parallel to the ecliptic, with the exception of the central one of the western group, which was a little towards the north. They were all about the same size, and shone with a much greater lustre than fixed stars of the same magnitude. January the 14th was cloudy, but next night he saw all four stars to the west of the planet, all nearly in the same right line, and increasing in size and brilliancy, according to their distance from Jupiter. And so he continued nightly, up to the 2nd of March, 1610, to make these observations, sixty-six of which are figured and described in the *Sidereus Nuncius*.[1]

The persistence of the relative distances between these four bodies and Jupiter in all their changes left no room for doubt that they accomplished with him, and in about twelve years, a revolution around the sun as a centre. Their own orbits round the planet were unequal in time, those nearest moving more rapidly than those more remote; while the most remote of all appeared to complete its revolution in about fifteen days.

During the Easter recess of 1610 at Padua, Galileo had an express invitation from the Tuscan Court, then at Pisa, to explain to the Grand Duke his discovery of the four satellites of Jupiter, which, in honour of the reigning family, he proposed to call Medicean Stars, after the four brothers Cosimo II, Francesco, Carlo, and Lorenzo. Cosimo II, who all his life showed a sincere attachment to his tutor, asked for and obtained the gift of the instrument with which this discovery was made; but Galileo quickly repented of his generosity. He evidently could not part with his 'old

[1] There are also (1) Diagram to illustrate principle of the telescope, (2) five drawings of the moon's superficies, (3) two diagrams on lunar measurements (4) Orion, (5) Pleiades, (6) Nebulosa Orionis, and (7) Nebulosa Praesepe.

discoverer', as he affectionately called it in after years; so, while always reserving it for the Grand Duke, he kept it near himself till his death, when it was handed over to Prince Leopoldo, brother of Ferdinando II.

Of its subsequent history little is known with certainty. It would appear that in Galileo's last years the instrument was accidentally broken. Then, in 1675, there is a record in the inventory of the effects of Cardinal Leopoldo de' Medici of a 'broken object-glass with which Galileo discovered the four new planets'; and in 1677 another record of its having been set in an ivory frame. It is now preserved, together with two telescopes, said to have been made by Galileo, and certainly of his time, in the Tribuna di Galileo at Florence, with many other precious relics of the period. Accurate measurements of it have been made quite recently by Professor Roiti of the University of Florence, as follows: Focal distance 1·70 metres, diameter 0·056 metre. One face has the curvature of a sphere with radius of 0·935 metre, and the other face is practically plane, having just a trace of convexity.[1]

### 12. *On Saturn* [2]

After completing his study of Jupiter, Galileo turned his glass to the other planets to see if they also had attendant moons. On the 25th of July, 1610, he was rewarded by another brilliant discovery—the phenomenon that we now describe as the ring of Saturn. To him, however, it did not appear as a ring, but as a triple star of which the central part was about three times larger than the laterals, and all three almost touching, and in a plane parallel to the zodiac. He made further observations in the autumn, when Saturn was well above the horizon, and, fearing that some one might forestall him, he announced the discovery in a brief letter, dated Padua, 30th July, 1610, to Belisario Vinta, at Florence, but begged him to keep it secret for a while. As a further precaution lest his claims should be forestalled he sent to friends in Italy and Germany a cryptogram of thirty-seven letters as follows:

SMAISMRMILMEPOETALEUMIBVNENUGTTAVIRAS

Kepler and other friends puzzled long over this anagram, the former thinking it had some reference to his favourite planet

[1] Cf. Favaro's *Intorno ai Cannocchiali costruiti ed usati da Galileo*, Venezia, 1901.

[2] Cf. Nat. Ed., vol. x, pp. 410, 474; xi, p. 439; xviii, p. 238.

Mars. At length, Giuliano de' Medici, Tuscan ambassador at the German Court, was charged by the Emperor Rudolph II to ask for the solution, to whom Galileo, replying 13th November, 1610, gave the following startling solution :

'Altissimum Planetam Tergeminum Observavi.'

'I have observed', he says, 'with great admiration that Saturn is not a single star but three together, which, as it were, touch each other. They have no relative motion, and are constituted in this form [see Fig. 7, i], the middle being much larger than the lateral ones. They are not strictly in the line of the zodiac, but rather parallel to the equinoctial line. . . . I have already discovered a court for Jupiter, and now there are two attendants for this old man, who aid his steps and never leave his side.'

The learned world had not yet had time to digest the surprising facts announced in the *Sidereus Nuncius*, when this asserted triple nature of Saturn again contravened the prevailing Aristotelian ideas. Continuing his observations, Galileo found that the lateral bodies did not retain the same apparent magnitudes. In fact, they had been gradually diminishing, although they appeared to be immovable, both with respect to each other and to the central body. They continued to grow less and less during the next two years, and towards the close of 1612 they vanished altogether! Horrified at this extraordinary phenomenon, and full of alarm for the consequences to himself when his Aristotelian opponents should come to hear of it, he thus wrote to Welser on December 1st, 1612 :

'Looking at Saturn within these last few days, I found it solitary without its accustomed stars, and, in short, perfectly round and defined like Jupiter, and such it still remains! Now what can be said of so strange a metamorphosis ? Are, perhaps, the two smaller stars consumed like spots on the sun ? Have they suddenly vanished and fled ? Or has Saturn devoured his own children ? Or was the appearance, indeed, fraud and illusion, with which the glasses have for so long mocked me and many others who have observed with me ?'

He continued, however, to conjecture that the two attendant stars would reappear after revolving with the planet, and that, by the summer solstice of 1615, they would be not only again visible, but more luminous and larger. And by the middle of 1615 he was able to verify his prediction, for the lateral stars were now reappearing (Fig. 7).

No change calling for special comment was noticeable until the summer of 1616, when he made a new observation relating to Saturn. In August of that year, writing to Prince Cesi, Galileo says:

'I cannot rest without signifying to your Excellency a new and most strange phenomenon observed by me in the last few days in Saturn. Its two companions are no longer two small and perfectly round globes, as they have hitherto appeared to be, but are now bodies much larger, and of a form no longer round, but, as shown in the annexed figure (see Fig. 7, III), with the two middle parts obscured, that is to say, the very dark triangular-like spaces contiguous to the middle line of Saturn's globe, which latter is seen, as always, perfectly round.' [1]

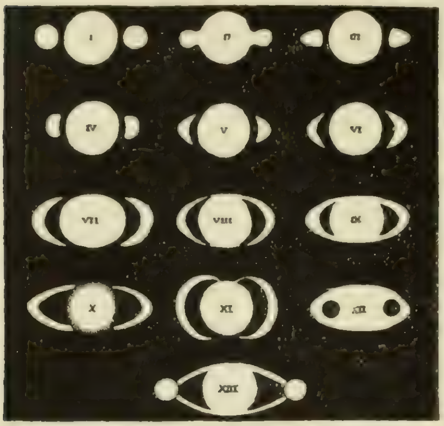



FIG. 7. EARLY DRAWINGS OF SATURN
From the *Systema Saturnum*.

Up to the last, Galileo made no announcement as to the precise nature of Saturn's appendages. He contented himself with describing what he saw, and, recognizing the incompleteness of his knowledge, and, perhaps, the inadequate power of his glass, he left it to the future to solve the problem. This was done by Christian Huygens in 1655. Working with a refracting telescope, magnifying 100 diameters, this astronomer not only saw and described the ring as a ring, but discovered one of Saturn's satellites.

### 13. *Venus, Mercury, and Mars* [2]

These discoveries stimulated yet further the interest of Galileo's grand-ducal pupil, and in June 1610 Cosimo II nominated him 'First Mathematician of the University of Pisa, and First Mathematician and Philosopher to the Grand Duke'. Galileo now returned to Florence. Here, on September 30, he made another astounding discovery in the heavens, namely, the occasional

[1] Cf. Favaro, 'Intorno all' Apparenza di Saturno osservata da Galileo nell'Agosto 1616' (*Atti del Reale Istituto Veneto*, February, 1901).

[2] Cf. Nat. Ed., vol. x, pp. 483, 499, 503; vol. xi, pp. 11–12.

crescent form of the planet Venus. After satisfying himself, by three months' observations, of its correctness, he announced the fact in a letter (11th December) to his friend Giuliano de' Medici at Prague, but concealed it again in an anagram as follows: 'Haec immatura a me iam frustra leguntur o y.' He did not, however, leave his friend long in perplexity, for on the 1st of January, 1611, he sent him the solution: 'Cynthae figuras aemulatur mater amorum.' 'That is, *Venus rivals the appearance of the moon*; for, being now arrived at that point of her orbit in which she is between the earth and the sun, and with only a part of her enlightened surface turned towards us, the telescope shows her in a crescent form, like the moon in a similar position.' Following her through the visible portion of her orbit, he had the satisfaction of seeing the illuminated part assume successively the crescent forms appropriate to his hypothesis.

It was with reason, therefore, that he laid stress on the importance of these observations, which established yet another fact obnoxious to the Aristotelians—a further resemblance between the earth and moon and one of the principal planets. As he had shown in *Sidereus Nuncius* that the earth, like the moon, is luminous only where exposed to the sun's rays, so this change of figure in Venus demonstrated that she and, probably, all the other planets were not luminous of themselves, but reflected the sun's light. Thence he concluded that they must all revolve round the sun—'a fact surmised by Pythagoras, Copernicus, Kepler, and their disciples, but that could not be proved by ocular demonstrations'. For it had always been a formidable objection to the Copernican theory that Venus and Mercury did not exhibit the same phases as the moon, which they should do if they revolved round the sun, and Copernicus himself had endeavoured to account for this by supposing that the sun's rays passed freely through the body of the planets.

Of similar changes in Mercury, the existence of which he inferred by analogy, he could observe nothing, because that planet's orbit does not take him far from the sun, and, in consequence, his small disk is always so resplendent that not even the best telescope could deprive him of his factitious rays.[1]

[1] The revolution of Mercury about the sun, which Galileo assumed, was confirmed twenty years later. Just before his death in 1630, Kepler predicted a transit of Mercury for the next year, and it was duly observed, on November 7, 1631,

The orbit of Mars being exterior to that of the earth, he is not subject to phases like the inferior planets Venus and Mercury, but in certain positions he assumes a gibbous appearance, like that of the moon a little before and after the full. Galileo recognized this feature, and, after four months' careful observation, he announced that 'when Mars is in quadrature, or the middle points of his path on each side of the sun, his figure varies slightly from a perfect circle. I dare not affirm that I can observe phases, but, if I mistake not, I already perceive that he is not always perfectly round.' He also observed that the apparent size of the planet varied according to its distance from the sun, being sixty times larger when in opposition than when in conjunction.

### 14. *On Sun-Spots* [1]

In consideration of the intense interest, friendly and otherwise, excited by these discoveries in Rome, Galileo thought it desirable to go there himself, and acquaint at first hand the *savants* and dignitaries of the Church with his work. It was not till March 23, 1611, that he was able to set out, provided with many letters of introduction, amongst them one from Michelangelo the younger (nephew of the great sculptor and painter) to Cardinal Barberini (afterwards Pope Urban VIII). He was received with distinction by princes and all the Church dignitaries, as well as by the learned laymen. Even those who discredited his discoveries, either through obstinacy or through fear of their results, were as eager as the true friends of science to see and hear this wonder of the age.

After exhibiting on several occasions all his recent discoveries, or 'celestial novelties' as they were called, a commission of four scientific members of the Roman College was appointed to examine them. Their report of April 24 was favourable on all points, and was considered as equivalent to an official *Imprimatur*. Pope Paul V granted him a long audience, and assured him of his unalterable goodwill; high dignitaries of the Church followed suit, and the Accademia dei Lincei elected him a member.

Immediately after the publication of the report of the com-

by Gassendi, who followed Kepler's instructions. Our own countryman, Horrocks, was the first to observe a transit of Venus, in 1639.

[1] Cf. Nat. Ed., vol. v, pp. 10–260; vol. vii, p. 372; vol. xiv, p. 299.

# SIDEREVS
## NVNCIVS

MAGNA, LONGEQVE ADMIRABILIA
Spectacula pandens, suspiciendaque proponens
vnicuique, præsertim verò
*PHILOSOPHIS, atq; ASTRONOMIS, quæ à*

GALILEO GALILEO
PATRITIO FLORENTINO
Patauini Gymnasij Publico Mathematico

PERSPICILLI
*Nuper à se reperti beneficio sunt obseruata in LVNÆ FACIE, FIXIS IN-*
*NVMERIS, LACTEO CIRCVLO, STELLIS NEBVLOSIS,*
*Apprime verò in*

QVATVOR PLANETIS
Circa IOVIS Stellam disparibus interuallis, atque periodis, celeri-
tate mirabili circumuolutis; quos, nemini in hanc vsque
diem cognitos, nouissimè Author depræ-
hendit primus, atque

MEDICEA SIDERA
NVNCVPANDOS DECREVIT.

VENETIIS, Apud Thomam Baglionum. M DC X.

*Superiorum Permissu, & Priuilegio.*

FIG. 8. Title-page of Sidereus Nuncius.

mission, Galileo announced yet another new discovery in the heavens, namely, dark spots on the body of the sun, which, towards the end of April 1611, he showed to several prelates and men of science in Rome. Describing these phenomena,[1] he states that at first he was undecided whether to explain the ever-changing form and position of the spots by supposing that the sun revolved, or by imagining that other and hitherto unknown planets revolved about the sun, and were visible only as spots on his disk. Further observation, however, led him to abandon the latter supposition and to announce positively that the spots were in contact with the body of the sun, where they were continually appearing and disappearing much as clouds about our earth. These observations were, in their consequences to Galileo, particularly unfortunate, as he thereby became embroiled with the powerful Jesuit party whose influence was one of the chief causes of his subsequent misfortunes.

A Jesuit father, Christopher Scheiner, Professor of Mathematics at Ingolstadt, claimed priority in the discovery of the sun-spots, asserting that early in 1611 he first noticed them and showed them to his pupils. Scheiner stated his case in three open letters addressed to Mark Welser, Chief Magistrate of Augsburg, though it is clear from these letters that at first he attached no importance to these appearances, and even thought them due to defects in his glasses. The spots he supposed to be caused by multitudes of little planets, revolving round the sun in an orbit inside Mercury, and producing the appearance of spots in crossing his disk.

On the publication of Scheiner's letters, Welser sent a copy to Galileo, requesting to be favoured with his opinions of the phenomena therein described. He replied in three letters dated respectively 4th May, 14th August, and 1st December, 1612. In the first letter (the autograph copy of which is now in the British Museum) he begins by saying that the phenomena are not illusions produced by the glasses, but veritable facts, which he himself had observed eighteen months before in Florence, and which he had shown to many people in Rome in April of the past year.[2] He

[1] *Discourse on Floating Bodies*, 1612, which see *infra*, p. 249.

[2] At the end of *Il Saggiatore* (1623), and in the Dialogue of 1632 (3rd Day), he states that he first observed the spots while still in Padua, and that he showed them to some friends. This would take the date back to the summer of 1610. The claim is supported by Fulgenzio Micanzio and Viviani. Galileo explains his

then proceeds to combat Scheiner's various and often contradictory assertions as to the nature of the spots, and their places and movements in relation to the body of the sun.

'I would say', he writes, 'that they are formed on the sun's superficies, that they are carried round with him in his rotation, remaining visible for about one-half month ; and that they may be something of the nature of our own clouds. Certainly, if our earth were self-luminous and surrounded by clouds, it would seem to a far-distant observer to have spots like those we see on the sun, now uniting, now separating, and now dissolving. They would follow her in her rotation, appearing very large at the centre of her disk, where their motion would be most rapid, and contracting towards the edges, where they would be smallest, and where also their velocity would be least.'

This he recognized as an effect of fore-shortening which would result if, and only if, the spots were on or very near the sun.

In the second letter he restates his views, adding some further particulars as to the constant, slow, and irregular changes in the form of the spots, and their varying density, being very dark at the centre, and less so towards the circumference—'a manifest proof of the sun's sphericity'. They are confined to a zone about the sun's equator, extending 28 or 29 degrees (in his third letter he says 29 or 30) on each side, beyond which they are never seen ; and, finally, they all have a common motion of rotation. From all these facts, and from the additional one, that often the same spot disappearing at one side reappears at the other, he concludes that the sun is a sphere, that it rotates on its axis from west to east, and that it performs one such rotation in about a lunar month. In an appendix he gives forty sketches of spots as observed from day to day during June, July, and August, 1612.

In his third letter, dated 1st December, 1612, Galileo notices some observations of Scheiner on Venus and the moon, and shows the falsity of his 'facts' and deductions ; he recurs to the sun-spots and adduces a further proof of the correctness of his own hypothesis in the behaviour of some very bright spots (*piazzette*, now called *faculae*). Some parts of the sun's disk are perceived

silence as to these earlier observations thus : 'Having regard to the extraordinary nature of the phenomena, so contrary to the received opinions, I judged it more prudent to wait until I had convincing proof. I prefer to be the last to produce a true conception than to anticipate others at the risk of having to unsay what I was in a hurry to affirm' (Nat. Ed., vol. v, p. 94).

to be brighter than the rest, and these parts appear to traverse the disk just as the other spots do. Now, were these very bright spots planets, as Scheiner would have it, they ought sometimes to appear beyond the sun's limb, but this they never do—an irrefragable proof that they are part and parcel of the sun himself.

After referring to various subjects, as the inhabitability of the planets, the supposed crystalline and transparent substance of the moon, the diversity of figure amongst the planets, and the periods of Jupiter's satellites (of which Scheiner had recently 'discovered' a fifth), he returns once more to the sun-spots and their general resemblance to clouds or smoke. We can, he says, imitate them in various ways, as, for instance, by dropping on a red-hot iron plate bits of bitumen. He supposes that the sun's light (and heat) may be sustained by a constant supply of new *pabulum*, which, like the bitumen, first gives off black smoke, which we see as spots. In a later letter, 23rd March, 1615, to Piero Dini, he refers to this idea. 'I suggested', he says, 'that these spots might be part of that pabulum (or rather the *débris* of it), of which, according to certain ancient philosophers, the sun has need for his sustentation.' [1]

These letters were ultimately published at Rome in 1613, at the expense of the Accademia dei Lincei, and under the title *Istoria e dimostrazioni intorno alle macchie solari*.[2]

### 15. *On Lunar Mountains* [3]

Soon after his return to Florence in June, 1611, Galileo wrote a series of letters on *The Inequalities of the Moon's Surface*, in defence of the views expressed in his *Sidereus Nuncius*. The moon was with him a stock subject for observation, the results of which he utilized in his astronomical works, or communicated in long letters to friends, notably to Griemberger and Gallanzoni in Rome, and to Welser and Bernegger in Germany. His last astronomical

[1] Newton and Buffon conjectured that comets might be the aliment of the sun, and, at present, a nearly similar explanation finds favour, viz. that streams of meteoric matter, varying in volume, are constantly pouring into the sun from the regions of space. Professor Turner of Oxford is the latest exponent of this hypothesis. See his paper in *Monthly Notices of R.A.S.*, December, 1913. Cf. Mayer, *Beiträge zur Dynamik des Himmels*, Heilbronn, 1848.

[2] Nat. Ed., vol. v, pp. 75–260.

[3] Cf. Nat. Ed., vol. iii, pp. 301, 313; x, pp. 461, 466; xi, pp. 141, 178.

discovery, towards the close of life and just before he became blind, was connected with the moon.

It had been asserted that, as the full moon always presented a well-defined outline, whether viewed with the naked eye or through a telescope, it was impossible that there could exist any inequalities around her circumference. Galileo maintained that the irradiation of the moon's light might be great enough to mask the asperities around her edge, and so effectually conceal the real nature of her surface. With respect to irradiation generally, he remarked that it increases with the brightness of the object. It is from this cause that the planets near the sun have a greater irradiation than those more remote. So intense is the irradiation of Mercury that it is impossible, even with the most powerful telescope, to deprive him of his brilliant corona. The same is true, though in a less degree, with respect to Mars. On the other hand, Jupiter, and especially Saturn, being more feebly illuminated by the solar light, lose their irradiation in the telescope, and disclose their true figures.

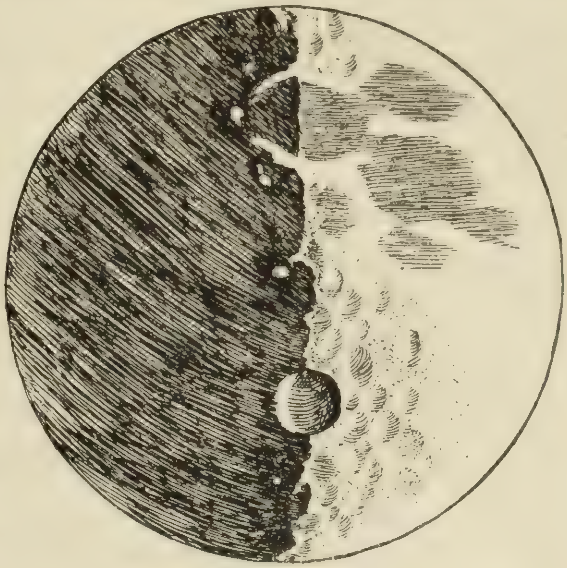


FIG. 9. The moon as seen by Galileo, 1609–10.
From *Sidereus Nuncius*.

With respect to Venus, when she is near her inferior conjunction, she in reality resembles the new moon; but such is the effect of her irradiation that she appears to the naked eye round like any other star. In this position, as the extent of the illuminated surface is small and the light is at the same time enfeebled by the obliquity of the surface, it is possible by means of a telescope to discern the real crescent appearance of the planet. When, however, she is near her superior conjunction, she presents a complete hemisphere of vivid light towards the earth of such intensity that even the most perfect telescope does not reveal to us her true figure. Galileo therefore contends that it is probable that

even the telescope will fail to efface the irradiation of the moon enough to disclose the eminences and cavities which may be situated near the edge of her disk.

His peripatetic opponents next tried to reconcile the old doctrine of a polished and perfectly plane surface with these new observations. Father Clavio doubted at first the reality of the inequalities, and thought that the appearances were due to inequalities in the reflecting power of the moon's substance. Other Aristotelians, as delle Colombe and Lagalla,[1] supposed that every part of the moon, which to us appears hollow, is, in fact, filled with clear crystal matter, thus preserving a round and smooth superficies, and it is this diversity of *substance*, with its more or less transparency, which gives the impression of inequality of *form*.

### 16. *Discussion of Habitability of Moon and Planets*[2]

Among the many burning questions to which the *Sidereus Nuncius* gave rise was that of whether the moon and planets were inhabited. This was openly discussed in Rome from 1611 onwards, and its ' manifest absurdity ' was used as an argument against the Copernican theory in general and Galileo's lunar observations in particular. If the moon, it was said, is so like the earth with land and water, mountains and valleys, and surrounded by an atmosphere, we may suppose that she too is the home of beings like ourselves. Again, if our earth be not the centre of the universe, but one of a number of planets, and a small one at that, then the other planets are inhabited like ours. The arguments thus resolved themselves into the syllogism—all planets are alike, the earth is a planet and is inhabited, *ergo* all planets are inhabited. The peripatetic philosopher, Lagalla, maintained this thesis in a public discourse in Rome in 1612. The same was gravely adduced by Scheiner in one of the numerous digressions in his letters on Sun-spots, and he insinuates that Galileo must hold the belief as a necessary consequence of his observations.

Yet far from admitting this view, Galileo took pains to show its impossibility. In his third letter to Welser on Sun-spots, he points out that for fifteen days continuously the moon is exposed to the scorching sun-rays, and for another fifteen consecutive days is

[1] Nat. Ed.. vol. iii, Part I.

[2] Cf. Nat. Ed., vol. xii, p. 240.

plunged in cold and darkness. A day of glare and heat equal to fifteen of ours, and a night of cold and darkness of equal length were, he pointed out, impossible conditions for life such as ours.[1]

### 17. *On Finding the Longitude at Sea*[2]

Very soon after he had discovered the satellites of Jupiter in 1610, Galileo began a work the difficulty and fatigue of which he has himself indicated by comparing it with the labours of Atlas. It was a series of observations on the periods of the satellites, 'with a view to drawing up tables so as to be able to predict all particulars of their situations, relations, and eclipses, and thus to have the means of determining at any hour of the night the longitude of the place of observation'. Kepler thought this enterprise so difficult as to be wellnigh impossible, and certainly Galileo did not find it easy. Notwithstanding many hundreds of observations in the next twelve months, repeated often twice and sometimes three times in a night, he had made little or no progress up to April, 1611. It was not until another year and more had elapsed that he was able to announce satisfactory results in his 'Discourse on Floating Bodies'. To show their close approximation, we give them here side by side with the modern figures.

PERIODS OF REVOLUTION

| | *Galileo.* | *Modern.* |
|---|---|---|
| Innermost satellite . . | 1 day 18 hrs. 30 mins. | 1 day 18 hrs. 29 mins. |
| Second ,, . . | 3 days 13 ,, 20 ,, | 3 days 13 ,, 18 ,, |
| Third ,, . . | 7 ,, 4 ,, 0 ,, | 7 ,, 4 ,, 0 ,, |
| Fourth ,, . . | 16 ,, 18 ,, 0 ,, | 16 ,, 18 ,, 5 ,, |

Our moon had already been suggested for the same purpose: it changes its position amongst the stars continuously, and, if at specified times throughout the night that position can be predicted, the mariner is able to determine his longitude. But, at the beginning of the seventeenth century, tables of the moon's positions were very inaccurate; and even its proximity to the earth was a disadvantage, for an observer at sea would get

[1] Cf. his letter to Giacomo Muti, February 28, 1616; Dialogue of 1632, first Day.

[2] Cf. Nat. Ed., vol. iii, Part II, *passim*; vol. v, pp. 415–25; vol. viii, p. 451; vol. xi, p. 321; vol. xii, pp. 256, 289, 311, 358, 392; vol. xiii, pp. 17, 370; vol. xiv, pp. 53, 91, 202, 349, 374.

a different view of it from one on land, and this difference in the moon's position amongst the stars would have to be allowed for. The errors due to these defects would, it was thought, be avoided if Jupiter's satellites were used instead of the moon. The much greater distance, the frequency of their eclipses (more than 1,000 yearly), and (it was expected) their suddenness seemed to promise success to Galileo's method. But in practice difficulties cropped up. First, there was the difficulty of observing such small objects as the satellites from a moving ship, and secondly, there was the want, common to both methods, of accurate time-keepers. To obviate the first he contrived what he called the *Celatone* or *Testiera* :

'I made', he says, 'for the use of our navy a kind of cap, fitted to the head of the observer, and supporting a telescope in such a way that it always points in the same direction as the free eye, so that an object viewed by the latter is also seen by the other eye through the telescope. A similar apparatus could be made and fixed on the shoulders and chest of the observer, to support a telescope of a power sufficient to show the satellites of Jupiter, and adjustable as in the case of the *Celatone*. When, then, the free eye is turned towards Jupiter, the other eye sees through the telescope not only the planet but its satellites.' [1]

With this contrivance and a chair for the observer, hung like a ship's compass on a binnacle, he hoped to overcome the first difficulty of unsteadiness ; while to remedy the second he had hopes of utilizing his early observations on the pendulum and applying it as an exact measurer of time.

In September, 1612, Galileo offered his method to the Spanish Government for use in their navy, but the proposal was not well received, and for the next four years he took no further steps in the matter. Now, during his visit to Rome in 1616, he reopened the negotiations through Count di Lemos, the Spanish Viceroy of Naples. Di Lemos was fully alive to the importance of the proposal, and promised to submit it to his Government. This was done, after much unaccountable delay, in March, 1617, and, in the same leisurely way, the proposal was discussed by the King in Council. To the various objections advanced Galileo replied, and even offered to go himself to Spain, but he could not, with all his enthusiasm, bring the Spanish Court to a decision. His

[1] Letter to Lorenzo Realio, June 6, 1637. From this it is clear that Galileo did *not* propose a *binocular* telescope as has sometimes been supposed.

disappointment was mitigated by his own sovereign taking up the method for use in the Tuscan navy. Its practical application, however, proved to be beset with so many difficulties that it soon fell into disuse.[1]

### 18. *On Floating Bodies* [2]

During the summer of 1611 the subject of floating bodies had been debated at one of the scientific parties which the Grand Duke liked to assemble round him. The general opinion was that of Aristotle, that the sinking or floating of a body in water depended upon its shape. Galileo undertook to show this view to be untenable,[3] and he embodied his arguments in a famous treatise, published in Florence in 1612, ' Discorso intorno alle cose che stanno in su l'Acqua, o che in quella si muovono '. In this work he restores the true principles of hydrostatics as laid down by Archimedes, alludes to the so-called hydrostatical paradox, first noticed by his contemporary Stevin of Bruges, and explains it on the principle of virtual velocities, as first clearly enunciated by himself in his treatise on the mechanical powers in 1594.

In the course of the discussion it was asserted that condensation is the effect of cold, and ice was mentioned as an example. Galileo retorted that ice is rather water rarefied than water condensed, the proof of which is that it always floats upon water. His opponents rejoined that this was due, not to the lightness of the ice, but to its incapacity, owing to its flat shape, to overcome the resistance which the water opposed to its sinking. Galileo denied this, and asserted that ice of any shape would float, and that if a flat piece were forced to the bottom it would, when left to itself, rise again to the surface.

The behaviour of ebony was then instanced, which in the shape

[1] In August, 1636, Galileo offered his method to the States General of Holland, but here again its practicability was questioned and for the same reasons as above. The negotiations dragged on wearily, and by the middle of 1640 came to an infructuous end, except for a collar of gold, as a mark of the Dutch Government's esteem, which Galileo refused, or, as is more likely, was not allowed by the Inquisition to accept.

[2] Cf. Nat. Ed., vol. iv, *passim* ; vol. xi, pp. 176, 304, 317.

[3] Cardinals Gonzaga and Maffeo Barberini (afterwards Pope Urban VIII) were among the guests, and the latter took Galileo's side in the discussion against the peripatetics led by Gonzaga.

of a ball sinks, but as a thin board floats when gently placed on the surface. To this he replied:

'The diversity of figure given to any solid cannot be the cause of its floating or sinking in water, though the breadth of the figure may indeed retard its velocity as well of ascent as of descent, and more and more in proportion to the breadth and thinness. If you examine carefully your thin boards of wood you will see that they have part of their thickness under water; and, moreover, you will see that shavings of ebony, stone, and metal, when they float, have not only broken the continuity of the water, but are *with all their thickness* under the surface, and this more and more according to their specific gravity.'

To show more clearly the non-resistance of water to penetration, he directs a cone to be made of wood or wax, and asserts that when it floats, either with its base or its apex in the water, the solid content of the part immersed will be the same, although the apex is by reason of its shape better adapted to overcome the resistance of the water to division. Shape, then, cannot be the cause of the buoyancy. He goes on:

'Now, let us return to the thin plate of gold or silver, or the thin board of ebony, and lay it lightly upon the water, so that it may stay there without sinking, and observe the effect. It will be seen that the board or plate is lower than the surface of the water, which rises up and makes a kind of rampart round it. But if it have already penetrated the water, and is of its own nature heavier than the water, why does it not continue to sink? My answer is, because in sinking till its surface is below that of the water, it carries with it the air, so that that which descends is not merely the board of ebony, but a compound of ebony and air, from which results a body no longer specifically heavier than the water, as was the ebony alone. But, gentlemen, we want the same matter; you are to alter nothing but the shape, and therefore have the goodness to remove this air. This may be done by washing the upper surface of the board, for the water, having once got between the board and the air, will run together, and the ebony will sink to the bottom. To demonstrate how truly the air does support these solids, I have found that when one of these bodies (which float when placed lightly on the water) is thoroughly bathed and sunk to the bottom, by carrying down to it a little air, without otherwise touching it in the least, I am able to raise and carry it back to the top, where it floats as before. To effect this I take a ball of wax, and with a little lead make it just heavy enough to sink slowly to the bottom, taking care that its surface is quite smooth and even. This, if put gently into the water, submerges almost entirely, there remaining outside

only a very little of the top, and, so long as it is thus joined to the air, the ball floats ; but if we take away the air, by wetting this top, the ball sinks to the bottom. . . . There is, therefore, a certain affinity between air and other bodies, which holds them united, so that they separate not without a kind of violence, just as between water and other bodies, for, in drawing such bodies wholly out of the water, we see it follow them, and rise sensibly above its level before it quits them.'

There is a confusion here between the phenomena of hydrostatic pressure and of capillary attraction or surface tension; and Galileo would, perhaps, have carried conviction more readily had he realized himself that the floating plate of metal indicated a natural property of liquids which deserved special investigation.

This book, like all his other works, encountered violent opposition from the 'book philosophers'; and it was in reference to this controversy that we have one of his fine *obiter dicta*. 'Ignorance', he said, 'had been the best teacher he ever had, since, in order to be able to demonstrate to his opponents the truth of his conclusions, he had been forced to prove them by a variety of experiments, though to satisfy his own mind alone he had never felt it necessary to make many.'

### 19. *First Encounter with the Inquisition*[1]

The uncompromising boldness with which Galileo published and supported his opinions, as we have seen, raised crowds of enemies against him. The Aristotelian professors, the Jesuits, the political churchmen, and those timid and respectable persons who at all times dread innovation, were drawn together against the man who threatened them with the penalties of too much knowledge. No longer able to combat his observations and deductions by asserting that the former were due to faults in his glasses or to apparatus 'devilishly designed to produce them', and that the latter were vainglorious and philosophically absurd, his enemies now took their stand on theology. After some months of underground agitation, Father Caccini, of the Dominican convent of San Marco, was the first to declare war openly, in a sermon from the pulpit of Santa Maria Novella in Florence. Preaching on the fourth Sunday in Advent (December 21, 1614), and selecting as his text Joshua x. 12, 13, and Acts i. 11, he opened with the words:

[1] Cf. Nat. Ed. vol. v, pp. 264, 281, 291, 309, 351; vol. xii, pp. 123, 183, 244, 277; vol. xix, pp. 272–421.

'Viri Galilaei, quid statis aspicientes in caelum?' Galileo explained and defended his position in long letters to Castelli, to Piero Dini, and to the Grand Duchess Cristina (December, 1614, to June, 1615), which together constitute a powerful *Apologia*.[1]

The pith of his argument is contained in the saying of Cardinal Baronius, which he quotes—'The Holy Spirit intended to teach us in the Bible how to go to heaven, not how the heavens go'. One or two passages from his letter to the Grand Duchess may be quoted:

'Methinks that in the discussion of natural problems we ought not to begin with the authority of passages from Scripture, but with sensible experiments and necessary demonstrations. . . . Nature being inexorable, acting only through immutable laws which she never transgresses, and caring nothing whether her reasons and methods of operating be or be not understandable by men, I hold that our conception of her works, which either sensible experience sets before our eyes, or necessary demonstrations prove, ought not to be called in question—much less condemned upon the testimony of Scriptural texts which may conceal under their words senses or meanings seemingly opposite. . . . To command professors of astronomy that they see to confuting their own observations and demonstrations is to ask the impossible. . . . As to opinions which are not directly articles of faith, certainly, no man doubts that his Holiness hath always an absolute power of admitting or condemning them; but it is not in the power of any creature to make them to be true or false otherwise than as they are.'

All through the year 1615 the agitation went on with unabated violence, denunciations were sent to the Holy Office, and the Inquisition began to make secret inquiries. At length the situation became so threatening, not only for our philosopher himself, but for science in general and the Copernican theory in particular, that Galileo, with the advice of friends, decided to take himself to Rome. Accordingly, on December 3, 1615, he set out, provided with cordial letters from the Grand Duke to his Ambassador Guicciardini, to Cardinals del Monte and Orsini, and others. After many weeks of alternating hopes and fears, the matter came officially before the Inquisition, with the result that on February 26, 1616, Cardinal Bellarmine, who had sat on the commission that investigated his discoveries on his former visit to Rome, was directed 'to summon before him the said Galileo, and admonish him to abandon the said opinions, and, in case of

[1] Cf. his letter to Francesco Ingoli, Nat. Ed., vol. vi, pp. 504–61.

refusal, the Commissary is to intimate to him, before a notary and witnesses, a command to abstain altogether from teaching or defending the said opinions, and even from discussing them. If he do not acquiesce therein, he is to be imprisoned '.

Here the process ended so far as Galileo was concerned, but on March 5 a decree was issued prohibiting and suspending certain writings. Amongst them the book of Copernicus (*Revolutions of the Celestial Orbs*, first published 1543) was ordered to ' be suspended until corrected '. Galileo appears to have taken the admonition with a very bad grace, if we are to believe Guicciardini, who was no friend of his. Writing to the Grand Duke on May 13, 1616, he says :

' Galileo seems disposed to emulate the monks in obstinacy. . . . It may be heard at any moment that he has stumbled into some new abyss or other. However, the heat will probably drive him from Rome before long, and that will be the best thing that can happen to him.' [1]

## 20. *The Tides* [2]

Yet amidst all these cares and worries Galileo's teeming mind was busy with many scientific problems. He had not been many days in Rome when a suggestion from Cardinal Orsini was enough to start him on a treatise on the Flux and Reflux of the Sea. This problem had from the earliest ages deserved its name—' The grave of human curiosity '. Some supposed the rise of the waters to be due to the influx of rivers ; others supposed the existence of subterraneous fires which periodically made the sea to boil up ; while others again attributed this boiling effect to changes of temperature in the sun and moon, or to variations in the amount of their light.

The ancient philosophers had vague ideas that the moon's attraction was the cause. ' The flow ', says Pliny, ' takes place every day at a different hour, according to the rising of the moon, which, with greedy draught, drags the seas along with it.' In modern times, Gilbert of Colchester had speculated on this connexion. ' There are ', he says, ' two primary causes of the motion of the seas—the moon and the diurnal revolution of the earth.

[1] Cf. letter from Mgr. Antonio Querengo to Cardinal d'Este, January 20, 1616 ; and, on the other side, Cardinal del Monte's letter to the Grand Duke, June 4, 1616.

[2] Cf. Nat. Ed., vol. v, pp. 373–95.

The moon does not act on the seas by its heat rays, or by its light. How, then? Certainly by the common or mutual effort of the bodies, or, to explain it by something similar, by their magnetic attraction.' The Jesuits of the celebrated college of Coimbra, as well as Marc Antonio de Dominis, once Catholic Archbishop of Spalatro, later Protestant Canon of Windsor, and finally himself a martyr of the Inquisition, and Kepler, the law-giver of the planets, all held much the same views. Kepler's words are worth quoting, as they embody his ideas of that universally mutual gravitation, which Borelli, Wallis, and Hooke after him saw more clearly, and which it was the glory of Newton to establish:

'Gravity is a mutual affection between cognate bodies towards union, similar in kind to the magnetic virtue, so that the earth attracts a stone more than the stone attracts the earth. Assuming the earth to be the centre of the world, then, wherever it may be, or wheresoever it may be carried by its animal faculty, heavy bodies will always incline towards its centre. If two stones be placed in any part of the universe near each other, and beyond the influence of a third cognate body, they, like two magnetic needles, will come together at an intermediate point, each approaching the other by a space proportionate to its mass. If the moon and the earth were not retained in their orbits by their animal force, or some other equivalent, the earth would mount towards the moon by one fifty-fourth part of the distance between them, and the moon would fall towards the earth by the other fifty-three parts, i.e. in the inverse ratio of their masses, and assuming their substances to be of the same density. If the earth should cease to attract its waters to itself, all the waters of the sea would be raised and flow towards the moon. This attractive virtue of the moon extends as far as the earth, and entices its waters, but, as she flies rapidly across the zenith, and the waters cannot follow so quickly, a flow of the ocean is occasioned in the torrid zone and towards the west.' [1]

Galileo's theory is that the tides are the visible effects of the terrestrial double movement, since they are the combined result of (1) the earth's daily rotation on its axis, and (2) the inequality of the absolute velocities of the various parts of the earth's surface in her revolution round the sun. In the whole range of the sciences over which Galileo left indelible marks of his genius, he

[1] *Astronomia Nova*, Prague, 1609. Ten years later Kepler abandoned these correct ideas, and depicted the earth in his *Harmonice Mundi* as a living monster whose whale-like mode of breathing occasioned the rise and fall of the oceans in recurring periods of sleeping and waking dependent on solar time.

made very few fundamental errors, and this is perhaps his most serious one.[1]

21. *On Comets and 'Il Saggiatore'* [2]

In August, 1618, three comets appeared, and the very brilliant one in the constellation of the Scorpion—one of the most splendid of modern times—especially attracted the attention of astronomers. Although this comet was visible until January, 1619, Galileo had little opportunity of observing it, as he was confined to bed nearly the whole time. However, on a small basis of observation he reflected much and imparted his views to his friends, amongst others to Mario Guiducci, a Florentine disciple. In May of the same year, 1619, Guiducci, in a Presidential address to the Accademia Florentina, gave the views of the Master—'not as demonstrative truth, but as plausible conjectures in a matter so abstruse'.

This address was published immediately under the title *Discorso delle Comete* (Florence, 1619), and from it we learn that Galileo did not regard comets as heavenly bodies analogous to the planets, but as atmospheric phenomena—columns of vapour which, rising from the earth to great heights, far beyond the moon (*spazi celesti*), became there temporarily visible by reflection of the sun's light. In fact, he classed them in the same category as rainbows and mock suns, thus for once agreeing with Aristotle, and opposing himself to the more correct views of his contemporaries, Tycho Brahé and Kepler.[3] Referring to some proposed observations for parallax he pointed out the difference in this respect between a fixed object, the distance of which may be calculated by two angular observations at a known distance apart, and atmospheric appearances like rainbows, which are simultaneously formed in different drops of water for each spectator, so that two observers in different places are, in fact, viewing different objects. He then

[1] Between 1630 and 1637 Galileo would seem to have changed his view and suggested that the moon's librations may be the cause of the tides—'which by the common consent of all philosophers are ruled by the moon'. See his letters to Fulgenzio Micanzio, 7th November, 1637, and to Alfonzo Antonini, 20th February, 1638.

[2] Cf. Nat. Ed., vol. vi, *passim*; vol. xi, p. 41; vol. xii, pp. 466, 494; vol. xiii, pp. 43, 46, 80, 90, 98, 106, 116, 142; vol. xviii, p. 423.

[3] Brahé thought that comets were the result of sudden condensations of the ether of space, while Kepler accounted them to be exhalations of the planets. Santucci held that they were produced in the heavens by the sun. See his *Trattato nuovo delle Comete*, Fiorenza, 1611. In later years, Galileo changed his opinion as to their 'probably terrestrial' origin.

warns astronomers not to engage in a discussion on the distance of comets before they assure themselves to which of these two classes of phenomena they are to be referred. The remark is in itself perfectly just, although the opinion which occasioned it is now known to be erroneous ; but it is questionable whether the few observations which up to that time had been made upon comets were sufficient to justify the censures which have been cast on Galileo on account of it.

In the course of Guiducci's essay, some opinions of the Jesuit Father, Orazio Grassi, were so indiscreetly handled as to raise the ire of the Jesuits' College at Rome.[1] Grassi, under the pseudonym of Lotario Sarsi, published an onslaught on Galileo's cometary theory in a book called *The Astronomical and Philosophical Balance* (1619)—a violent pamphlet full of abuse of Galileo and his school. Friends, like Prince Cesi and Mgri. Ciampoli and Cesarini, now advised that the master himself should take up the fight ; but ill health and the troubled state of the religious and political horizons prevented the appearance of his reply for four years. At length, on October 19, 1622, he sent the manuscript of *Il Saggiatore* (The Assayer) to Mgr. Cesarini in Rome, and for five months it passed from hand to hand among the members of the Accademia dei Lincei, who examined it carefully and (with the author's consent) altered some passages which might possibly have given a handle to his enemies. The Papal *Imprimatur* was granted on February 2, 1623, and the book appeared at the end of October with a dedication to Pope Urban VIII, and under the auspices of the Accademia dei Lincei. This celebrated work is a masterpiece of dialectics, for the author not only dexterously avoids the snares laid for him by Father Grassi and his abettors, but brings defeat and ridicule upon them at every turn. He takes, in order, the mistakes of his adversary, his false citations and false deductions, his errors of logic, of geometry, of physics, of astronomy, and exposes them all ; and this is done so courteously and in such sparkling style that *Il Saggiatore* deserves its reputation as a model of dialectic skill, and an ornament of classical Italian literature.

The book (Fig. 10) was a great success, but it intensified the

[1] Soon after the appearance of the comets, a discussion upon them took place in the Collegio Romano. It was published early in 1619 under the title *De Tribus Cometis anni 1618 : Disputatio Astronomica*, &c. It is interesting to note that Mazarin, then a boy of sixteen, took part in this discussion.

bitterness of the Jesuitical party, and the General of the Order forbade the members to speak of it, even among themselves. It is important to note that the Pope was delighted with it, and had it read aloud to him at table. Early in 1625 the book was denounced anonymously to the Inquisition as a veiled defence of the Copernican doctrines, and a movement was begun to have it prohibited, or, at least, 'corrected'. This attempt, however, failed, and brought only further discredit upon the agitators.

## IV. The Trial and Abjuration

### 1. *Galileo's Plea for Copernicanism*

On the election of Cardinal Maffeo Barberini to the Papacy as Urban VIII, August 8, 1623, Galileo conceived the idea of going to Rome to offer his congratulations in person, and to use his influence with the new Pope to obtain, at least, toleration for the Copernican doctrines, now no longer subject to the weighty opposition of Cardinal Bellarmine, who died two years before. Remembering the warmth of Barberini's regards for him while Cardinal, Galileo had much to hope from a Pontiff so enlightened. He was encouraged, moreover, by hopeful reports from friends in Rome. Prince Cesi, writing October 21, 1623, was able to tell him: 'Under the auspices of this most excellent, learned, and benignant pontiff science must flourish. . . . Your arrival will be welcome to his Holiness. He asked me if you were coming and when, and, in short, seems to love and esteem you more than ever.' Rinuccini, Cesarini, Ciampoli, and others wrote in the same strain. Ciampoli, the Pope's private secretary, wrote on March 16, 1624: 'It is certain that the longer your coming is deferred the more it is desired by all those gentlemen who esteem you, and keep you green in their memory. You will find in the Holy Father no ordinary affection towards your person.' Ill-health, however, and then the bad weather and worse roads, intervened, but, at length, Galileo set out for the Eternal City on April 1, 1624.

All Rome was aware of the favour in which the Pope held Galileo, and his letters express great satisfaction with his reception, but as regarded the object which was nearest his heart, he made no progress. Within six weeks he had had six long interviews with Urban VIII, who always received him most affably, and allowed him to bring forward all his arguments in support of the Copernican theory; but all to no purpose; for while the Pope

listened to his arguments, he would not grant his entreaties for even a passive toleration of the new doctrines.

Finding that his efforts to get the decree of March 5, 1616, revoked were of no avail, Galileo resolved with a heavy heart to return home, though the Pope had loaded him with favours which must have seemed like mockeries. His Holiness promised him a pension for his son, and sent a picture for himself; then two medals—one of gold and one of silver, and quite a number of Agnus Dei! Not content with these marks of favour, he addressed an official letter to the Grand Duke, on June 8, in which, to the no small chagrin of Galileo's enemies, his Holiness not only did full justice to our philosopher's services to science, 'the fame of which will shine on earth so long as Jupiter and his satellites shine in heaven', but laid special stress on his religious sentiments. Yet Galileo departed with the object of his visit unsatisfied.

### 2. *Dialogue on the Two Chief Systems of the World, the Ptolemaic, and the Copernican* [1]

Nevertheless, from various indications in the ecclesiastical world in the next two years, 1624–6, Galileo was led to think that the advocates of Copernicanism had now little to fear, provided that the defence was so circumspectly handled as not to outrage the Inquisition's decree of March 5, 1616, which condemned the doctrine not as 'heretical', but only as 'rash'.[2] He resolved to push on to completion a work he had contemplated as far back as 1610. This was to be entitled *A Dialogue on the Flux and Reflux of the Tides.* From 1626 to 1630 he was almost entirely engaged on this work, but was interrupted by frequently recurring illnesses and family troubles.[3]

At length, by the beginning of 1630, he had practically completed this Dialogue, and in announcing the fact to his friend

[1] Cf. Nat. Ed., vol. vii; vol. xiii, pp. 104, 236, 260–4, 365; vol. xiv, pp. 49, 64–70, 79, 97, 120, 150–67, 278–85, 331; vol. xix, pp. 327–30.

[2] Indeed it was known that Urban VIII, as Cardinal, did not approve of that decree. Early in 1630, when discussing it with Campanella he said: It was never our intention and, if it depended upon us, that decree would not have been issued' (Nat. Ed., vol. xiv, p. 88).

[3] About this period he was often consulted, with others, on hydraulic questions connected with the flooding of Tuscan rivers (Nat. Ed., vol. vi, p. 613). Cf. Cambiagi, *Raccolta d'Autori che trattano del moto dell' Acqua*, Firenze, 1765–74; Napier, *Florentine History*, London, 1847.

Prince Cesi, he expressed his intention of going himself to see to the printing at Rome, where the state of affairs seemed still favourable for this enterprise. Galileo's disciple, Castelli, had been called from Pisa in March, 1626, to be mathematician to the Pope, and enjoyed great consideration with all the members of the Barberini family. This life-long friend, like Cesi, approved the design, and informed our philosopher (February 9) that Father Niccolo Riccardi, another old pupil and now chief censor of the press, had promised his assistance. Filled with hope and with his manuscript complete, Galileo at length set out on May 1 in a Court litter, and travelling fast arrived in Rome on the evening of the 3rd. He had a long audience of the Pope, and wrote on May 18 to Florence in high spirits: 'His Holiness has begun to treat my affairs in a way that permits me to hope for a favourable result'. But Galileo was far too sanguine, for toleration, to say nothing of the recognition, of the Copernican theory was as far off as ever. Urban VIII would not object to the publication, but certain conditions must be fulfilled. The title of the book, *Dialogue on the Flux and Reflux of the Tides*, was misleading and must be altered. The subject, being a discussion of the relative merits of the Copernican and Ptolemaic systems, should be indicated in the title. The subject, moreover, would have to be treated from a purely hypothetical standpoint, and this fact must be clearly set forth in a preface. Then, the book must conclude with an argument which the Pope communicated to Galileo in 1624, and which his Holiness considered unanswerable. That argument was as follows: 'God is all-powerful; all things are therefore possible to Him; *ergo* the tides cannot be adduced as a necessary proof of the double motion of the earth without limiting God's omnipotence.' Rather than forgo the publication of a work towards which he had laboured and thought for over thirty years, Galileo consented to these conditions.

Meanwhile the manuscript had been submitted to Father Riccardi, who, after certain revisions and alterations, granted permission for the printing in Rome. Thus, by the end of June, 1630, Galileo was back in Florence with his manuscript revised and corrected, and with the ecclesiastical *Imprimatur* for its publication in Rome, on the understanding that a preface and conclusion would be added in accordance with the Papal wish. Publication seemed imminent, yet for another twenty months our author was tormented with obstacles and delays on the part of

the censors, now in Rome, now in Florence. It would be little profitable to set out in detail these quibbling complications; but the work was at last issued from the Florentine press in February, 1632. It was in Italian, was dedicated to Ferdinando II of Tuscany, and bore the title, 'Dialogue of Galileo Galilei, Lyncean, Mathematician Extraordinary of the University of Pisa, Philosopher and First Mathematician of the Most Serene Grand Duke of Tuscany; where in meetings of four Days are discussed the Two Chief Systems of the World, indeterminately proposing the Philosophical and Natural arguments, as well on one side as on the other'.

The dialogue is carried on by three interlocutors, of whom two adduce the scientific reasons for the double motion of the earth, while the third tries to defend the opinions of the Ptolemaic and Aristotelian schools. Galileo gave to the defenders of the Copernican doctrine the names of two of his warmest friends, both long dead—Filippo Salviati of Florence (d. 1614), and Giovanni Francesco Sagredo of Venice (d. 1620). Salviati is the special advocate of the Copernican doctrine; Sagredo is witty, impartial, and open to conviction. To him are allotted such objections as have real force, as well as lively illustrations and digressions, which would be inconsistent with the gravity of Salviati's character. Simplicio, a name borrowed from the noted Cilician commentator of Aristotle who wrote in the sixth century, is a confirmed Ptolemaist and Aristotelian, and produces, as occasion requires, all the arguments of the peripatetic school; and as these fail to convince he has recourse to all the arts of sophistry.

The condition that the Copernican doctrine is to be treated as an hypothesis is ostensibly complied with. If Salviati or Sagredo show the untenableness of some Ptolemaic axiom, or add a stone to the Copernican structure, a remark is interpolated by one or other to weaken the effect. When we remember its history we cannot be surprised that the preface or introduction has no logical agreement with the contents of the Dialogue. The conclusion agrees no better than the preface with the body of the work. At the end of the fourth Day, which is almost wholly taken up with the tides, comes naturally the Pope's 'unanswerable' argument of 1624. Salviati treats it accordingly: 'It is', he says, 'an admirable and truly angelic argument, and perfectly in accord with that which, coming from God Himself, permits us to discuss the constitution of the world—doubtless with the view

of preventing by exercise the diminution and enfeeblement of our intellectual faculties, while withholding from us the power of fully comprehending the works of His hands.'

The contents of the Dialogue can be given only in outline. Salviati opens by defining its object, which is to examine all the physical arguments evoked for and against their opinions by the defenders of Aristotle and Ptolemy on the one hand, and of Copernicus on the other. In a few words, the Aristotelian doctrines amounted to a statement that, whereas things earthly are imperfect and full of change, things heavenly are eternal, unchangeable, and perfect. Salviati proves that this statement, in the spirit in which it was usual to accept it, is untenable. The telescope shows imperfections on the sun's surface, while the recently observed new stars (of 1572 and 1604) are instances of change in the heavens. He thus prepares the way for a still wider departure from Aristotelian theory ; he insists that the time has come to consider the nature of the world *de novo*, suggesting that Aristotle, had he the opportunities which the telescope afforded, would himself have realized the inadequacy of his own teaching.

Salviati proceeds to point out certain resemblances between the earth and moon and the more distant heavenly bodies. It was admitted that the moon shines only in virtue of the sunlight falling on her. The idea that the earth might similarly appear luminous to an inhabitant of the moon is less familiar and less readily accepted. And yet the visibility of the moon during a total eclipse of the sun, and the appearance 'of the old moon in the arms of the new' (as we now speak of it), are due most probably to reflected earth-light. The phenomena of Venus's phases are shown to be similar to those of the moon, and may be explained as due to the same cause. Venus, then, like the moon, owes her brilliance to the sun's light falling on her. The same probably applies to Mercury and Mars. The obvious inference seems to be that all these heavenly bodies are not so unlike the earth as men had thought. Points of resemblance there certainly are, and there may be many more which the distance of the planets alone prevents us from discovering. Salviati then refers to the spherical form common to earth, sun, moon, and planets, and suggests the existence of a common cause for that shape.

Here follow some remarks which show the idea of universal

gravitation hovering in Galileo's mind. He perceived the analogy between the power which holds the moon in the neighbourhood of the earth and compels Jupiter's satellites to circulate round their primary, and that attractive power which the earth exercises on bodies at its surface ; but he failed to conceive the combination of central force with initial velocity, and was disposed to connect the revolutions of the planets with the axial rotation of the sun—a notion which tended more towards Descartes' theory of vortices than towards Newton's theory of gravitation.

Having laid stress on the resemblance of earth and planets, Salviati proposes for them all a similar motion round the sun—one of the two main points of the Copernican theory. He shows how the apparent paths of all the planets can be thus explained, and in a far simpler way than by the Ptolemaic formula. On the Copernican hypothesis all motions of revolution and rotation take place in the same direction from west to east, whereas the Ptolemaic system requires some to be in one direction and some in another. A glance through a telescope turned towards Jupiter shows a family of small bodies circling round a great planet ; here one could see on a small scale the very thing that Copernicus had described as going on in the case of planets and sun.

On the second Day the discussion passes to the other chief point in the Copernican hypothesis—that the apparent daily motion of the stars is really due to the daily rotation of the earth on its axis. Here he breaks entirely new ground in his treatment of motion. His great discovery, which threw a new light on the mechanics of the solar system, was substantially Newton's first law of motion—' Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it is compelled by force applied to it to change that state '. Putting aside any discussion of this ' force ', a conception first defined by Newton, and only imperfectly grasped by Galileo, we may interpret the law as meaning that a body has no more inherent tendency to diminish its motion or to stop than it has to increase its motion or to start, and that any alteration in either speed or direction is to be explained by the action on it of some other force. As it is impossible to isolate a body from all others, we cannot experimentally realize the state of things in which it goes on moving indefinitely in the same direction and at the same rate ; it may, however, be shown that the more we remove it from external

influence the less alteration there is in its motion. Galileo illustrates this idea by a ball on an inclined plane. If the ball is projected upwards its motion is retarded ; if downwards it is continually accelerated. This is true if the plane be fairly smooth and the inclination not very small. If now we imagine the experiment performed on an ideal plane, which is perfectly smooth, we should expect the same results, however small the inclination. Consequently, if the plane be quite level, so that there is no distinction between up and down, we should expect the motion to be neither retarded nor accelerated, but to continue without alteration.

Other more familiar examples are given of the tendency of a body when once in motion. This principle of motion being once established, it becomes easy to deal with several common objections to the motion of the earth. The case of a stone dropped from the top of a tower, which, if the earth be moving rapidly from west to east, might be expected to fall to the west, is compared to that of a stone dropped from the masthead of a moving ship. It is, therefore, entirely in accord with theory that the stone should fall as it does at the foot of the tower.

No objections to the hypothesis of the earth's rotation being found tenable, it is shown by Salviati how much more simple is the real motion proposed than the supposition that the universe revolves daily round a fixed earth. 'To make the universe revolve', he says, 'in order to maintain the immobility of the earth is as little reasonable as to require, in order to see Venice from the top of the Campanile, that the whole panorama should move round the spectator instead of his simply moving his head.'

The primitive notion of the stars as fixed in a crystal sphere had been long overthrown. And, supposing that they were distinct and independent bodies, it was difficult to imagine laws controlling their motion about a fixed earth that should result in revolutions timed uniformly for all and at the same time of enormous rapidity. Salviati makes the improbable practically impossible by referring to the phenomenon now known as the 'precession of the equinoxes', in virtue of which the direction of the earth's axis in space moves slowly, completing a revolution in about 26,000 years. As a consequence of this change, some of those stars, which in Ptolemy's time were describing very small

circles and, therefore, moving very slowly, must now be describing larger circles at a greater speed, and vice versa. The system of stellar motions that would be necessary to account for all this would be inconceivably complex, whereas, on the Copernican theory, they are adequately explained by the rotation of the earth and a simple displacement of its axis of rotation.

A great part of the third Day is devoted to the question of stellar parallax. In this lay one of the most serious objections to the Copernican theory. If it was true that the earth swept round the sun in an orbit some two hundred million miles across, then it must follow that at one time of the year we should get a different view of the arrangement of the stars from that obtained six months later, when the earth was at the opposite point of its orbit. The nearer stars should undergo displacements in their apparent positions relative to those more distant. The answer to this was that these displacements probably did take place, but were too minute to be detected. But this answer, though strictly true, implied that the distances of even the nearest stars were great beyond all comprehension; and this in turn implied that the visible size of the stars indicated a real size of inconceivable dimensions. The latter difficulty was reduced by Salviati's assertion that the visible size of a star was an optical illusion; the telescope showed the stars to be sharp points, in contrast to the planets which though small to the eye really did possess visible dimensions. But the former difficulty remained, and nearly two centuries passed before Bessel made the first rough measurement of a stellar parallax. His method was essentially that suggested in the Dialogue, though the results obtained indicated for the star 61 *Cygni* a distance which would have astonished even Galileo himself.

Towards the end of the third Day reference is made to an annual rotation of the earth about an axis perpendicular to the plane of its motion, as postulated by Copernicus. But this third rotation is an unnecessary complication introduced by confusion in geometrical thought. That the actual state of things is quite simple Salviati illustrates by a reference to the motion of a ball floating in a basin of water. If the basin be held in the hand, the ball floating at or near the centre, and the experimenter turn round steadily on his feet, holding the basin in front of him, the ball remains in a position which is unaltered with reference to the walls and furniture of the room, although with reference to the

man supporting the basin it might be said to have spun once completely round. And so with regard to the annual rotation spoken of by Copernicus, ' What other is the earth than a globe librated in tenuous and yielding air ? '

The fourth Day is devoted to an examination of the cause of the tides, and is a development of his letter on the same subject to Cardinal Orsini in 1616. It is a singular circumstance that the argument on which Galileo mainly relied, as furnishing a physical demonstration of the truth of the Copernican theory, rested on a misconception. The tides, he says, are of three kinds—daily, monthly, and yearly. The first and principal ones are a visible effect of the terrestrial double movement, since they are the combined result of (1) the earth's daily rotation, and (2) the inequality of the absolute velocities of the various parts of the earth's surface in its revolution round the sun. The monthly tides depend in a secondary way on the moon's motion, and the annual tides, also in a secondary way, on the sun's action. These bodies, according to their positions relatively to the earth and to each other, produce inequalities in the earth's movements, and these inequalities are the cause of the monthly and yearly tides. To such notions Galileo attached capital importance, and he was inclined to ridicule Kepler's suggestion that the attraction of the moon was in some way the cause of the phenomenon. The influence of the moon on the tides had been recognized from ancient times, but a scientific explanation was not to be expected until the law of universal gravitation had been fully realized. Indeed, even now, with all the resources of modern science, the problem cannot be said to be completely solved.

This last part of the Dialogue is therefore of little value, and may be passed over. The chief work was to establish the Copernican theory, which, first promulgated in the days when human vision was unaided, had been found by Galileo to be supported by all the evidence that could be gathered by means of his telescope. The problem—if it may be still said to exist—takes a different form at the present day. So far are we now from the pre-Copernican theory of a fixed earth, that we look upon no single object in the universe as fixed. The sun itself has its motion amongst the other visible stars, and the present direction and rate of that motion are roughly known. Accordingly, the alternative which offered itself to the controversialists of Galileo's day, that

either the sun or the earth is stationary, does not concern us any more; both of these bodies are moving.

### 3. *Galileo's Second Encounter with the Inquisition. His Trial and Abjuration*, 1632–3 [1]

The publication of the *Dialogue on the Two Chief Systems of the World* raised a tumult in the ecclesiastical world, and especially among the Jesuits, who now resolved to pursue the author with the utmost energy. They claimed the monopoly of instruction and the first rank in the learned world, and were jealous of all intruders. Galileo was in every way inconvenient to them, and the more obnoxious in that he had already measured swords with distinguished members of the Order, Fathers Scheiner and Grassi. And now appeared his Dialogue, in which some old sores are reopened. The book was, therefore, denounced as a defence of Copernicanism under the flimsiest of disguises, as a gross violation of the admonition and decree of 1616, as an insult to the Pope himself. He was Simplicio—the Simpleton! Was not his unanswerable argument of 1624 put into the mouth of a simpleton, dragged in at the end, and summarily dismissed with a pious ejaculation? Galileo was charged with daring, after solemn warning, to interpret the Scriptures to his own ends. It was bad enough, they said, to upset the old beliefs and spoil the face of nature with his celestial novelties, but at least he must be taught to leave the Bible alone. In this he was rebellious against Mother Church, and, further, deceitful in that he obtained the *Imprimatur* by suppressing material facts in his dealings with the censors. Although the safety of the Church and the vindication of its decrees were the ostensible reasons for the subsequent proceedings, it would not be far from the truth to say that revenge for an assumed insult was the primary and determining factor. Urban VIII and many of the high dignitaries of the Church, if not Copernicans at heart, were indifferent and cared little one way or the other.[2] As regards the Pope himself, we have seen evidence of his affection for Galileo and of his liberal sentiments, of his relish for the works on Sun-spots, on Floating Bodies,

[1] Cf. Nat. Ed., vol. xiv, pp. 372, 402; vol. xvi, various; vol. xix, pp. 272–421.

[2] Even members of the Collegio Romano, including his old antagonists Fathers Scheiner and Grassi, were Copernicans in disguise. Cf. Favaro's *Adversaria Galileiana*, Serie Seconda, Padova, 1917, p. 27.

*Il Saggiatore*, &c. Then, we have his statements (1) that the Copernican doctrine is not heretical but only rash, and (2) that if it rested with him the decree of 1616 would never have been issued. All this seems to show that, if the question of a personal insult had not arisen, the Dialogue would at the worst have been put on the Index, as was the book of Copernicus, in 1616, 'until corrected'.

Early in August, 1632, the sale of the book was prohibited and its contents submitted to a special commission, who reported, after a month's session :

'1. Galileo has transgressed orders in deviating from the hypothetical standpoint, by maintaining *decidedly* that the earth moves and that the sun is stationary. 2. He has erroneously ascribed the phenomena of the tides to the stability of the sun and the motion of the earth, which are not true. 3. He has been deceitfully silent about the command laid upon him in 1616, viz. to relinquish altogether the opinion that the sun is the centre of the world and immovable and that the earth moves, nor henceforth to hold, teach, or defend it in any way whatsoever, verbally or in writing.'

On September 23 Galileo was ordered to appear in the course of the following month before the Commissary-General of the Holy Office in Rome. His friends pleaded his age and infirmities and the inclemency of the season, and begged that he might therefore be interviewed in Florence, but to no purpose. 'In Rome he must appear, and as a prisoner in chains if he will not come willingly', came the answer. To avert the most extreme measures the Grand Duke caused him to be informed (January 11, 1633) that it was at last necessary to obey the orders of the supreme authorities at Rome, and, in order that he might perform the journey more comfortably, Grand-ducal litters and a trustworthy guide would be placed at his disposal, and he would be lodged in Rome in the house of the Grand Duke's Ambassador. The pitiful impotence of an Italian ruler of that day in face of the Roman Church is painfully obvious in this decision. The Sovereign does not dare to protect his subject—even his old and respected tutor, but gives him up to the Inquisition, as if he were an alien malefactor.[1]

[1] The Venetian Republic was the only State in Italy that would have asserted its independence, as it had often done, and would have refused to hand over one of its officials to the Roman power. Indeed, when these proceedings began, Francesco Morosini of Venice offered to reinstate him in his old chair at Padua on any conditions that he chose to make, and to print his Dialogue in Venice.

On January 20, 1633, he left Florence, halted twenty days in great discomfort at the frontier on account of quarantine, and arrived in Rome on February 13. For the next four months the proceedings dragged on. He himself was inclined to hold out, his friends besought him to submit, he was ruthlessly haled backwards and forwards by his judges, questioned here and threatened there—the threat being a 'rigorous examination', an euphemism for physical torture. At last, on June 22, sentence was pronounced, and Galileo made his pitiful abjuration. After setting out in detail his various delinquencies, the Inquisitors conclude:

'Therefore, having seen and maturely considered the merits of your case, with your confessions and excuses, and everything else which ought to be seen and considered, we pronounce, judge, and declare that you have rendered yourself vehemently suspected by this Holy Office of heresy, in that (1) you have believed and held the doctrine (which is false and contrary to the Holy and Divine Scriptures) that the sun is the centre of the world and that it does not move from east to west, and that the earth does move and is not the centre of the world; and (2) that an opinion can be held and defended as probable after it has been decreed contrary to the Holy Scriptures, and, consequently, that you have incurred all the censures and penalties enjoined in the sacred canons and other general and particular codes against delinquents of this description. From this it is Our pleasure that you be absolved provided that, with a sincere heart and unfeigned faith, in Our presence you abjure, curse, and detest the said errors and heresies, and every other error and heresy contrary to the Catholic and Apostolic Church of Rome, and in the form that shall be prescribed to you. But that your grievous and pernicious error may not go altogether unpunished, and that you may be more cautious in future, and as a warning to others to abstain from delinquencies of this sort, We decree that the book *Dialogue of Galileo Galilei* be prohibited by public edict, and We condemn you to the prison of this Holy Office for a period determinable at Our pleasure, and by way of salutary penance We order you during the next three years to recite, once a week, the seven penitential psalms, reserving to Ourselves the power of moderating, commuting, or taking off the whole or part of the said punishment or penance.'

In conformity with this sentence, Galileo was made to kneel before the Inquisition, and make the following abjuration:

'I, Galileo Galilei, son of the late Vincenzio Galilei of Florence,

aged seventy years, being brought personally to judgement, and kneeling before you, Most Eminent and Most Reverend Lord Cardinals, General Inquisitors of the Universal Christian Republic against heretical depravity, and having before my eyes the Holy Gospels which I touch with my own hands, swear that I have always believed, and, with the help of God, will in future believe every article which the Holy Catholic and Apostolic Church of Rome holds, preaches, and teaches. But because I have been enjoined by this Holy Office altogether to abandon the false opinion that the sun is the centre and immovable, and been forbidden to hold, defend, or teach the said false doctrine in any manner; and because, after it had been signified to me that the said doctrine is repugnant to the Holy Scripture, I have written and printed a book, in which I treat of the same condemned doctrine, and adduce reasons with great force in support thereof without giving any solution, and therefore have been judged grievously suspected of heresy, that is to say, that I held and believed that the sun is the centre of the world and immovable, and that the earth is not the centre and is movable, I am willing to remove from the minds of your Eminences, and of every Catholic Christian, this vehement suspicion rightly entertained towards me. Therefore, with a sincere heart and unfeigned faith, I abjure, curse, and detest the said errors and heresies, and generally every other error and heresy contrary to the said Holy Church, and I swear that I will never more in future say, or assert anything, verbally or in writing, which may give rise to a similar suspicion of me; and that if I shall know any heretic, or any one suspected of heresy, I will denounce him to this Holy Office, or to the Inquisitor and Ordinary of the place in which I may be. I swear, moreover, and promise that I will fulfil and observe fully all the penances which have been or shall be laid on me by this Holy Office. But if it shall happen that I violate any of my said promises, oaths, and protestations (which God avert), I subject myself to all the pains and punishments which have been decreed and promulgated by the sacred canons and other general and particular constitutions against delinquents of this description. So, may God help me, and these His Holy Gospels which I touch with my own hands.

'I, the above-named Galileo Galilei, have abjured, sworn, promised, and bound myself as above, and, in witness thereof, with my own hand have subscribed this my abjuration, which I have recited word for word, in Rome, in the Convent of Minerva, this 22nd June, 1633. I, Galileo Galilei, have abjured as above with my own hand.'

While the older writers generally go to one extreme and say that Galileo was tortured, thrown into a dungeon for years, or

for the rest of his life, and was in physical fact a martyr, some recent authors have gone to the other extreme, and aver that he had no claim to much sympathy, that he had brought his troubles on himself by want of tact and temper. Others, again, blame him for not 'seeing this thing through'. Brewster, for example, compares him to the Christian martyrs, and finds him sadly degenerate. 'Had Galileo', he says, 'but added the courage of the martyr to the wisdom of the sage; had he carried the glance of his indignant eye round the circle of his judges; had he lifted his hands to Heaven, and called on the living God to witness the truth and immutability of his opinions, the bigotry of his enemies would have been disarmed, and Science would have enjoyed a memorable triumph.' Perhaps; but perhaps, on the other hand, his judges, instead of being cowed by the glance of his eye, would have delivered him to the stake, as they did Giordano Bruno earlier in the century (1600), and Marc' Antonio de Dominis only eight years before. Revealed truth may require its martyrs, and perhaps the blood of the martyrs may be the seed of the Church, but scientific truth is not thus established.[1]

After his sorrowful drama had concluded, Galileo was led back to the buildings of the Holy Office. And now that he and the Copernican theory were condemned with all the terrifying forms of the Inquisition, the Pope's wounded vanity was soothed, and he gave the word for a little mercy. Galileo was not to be kept in the prison of the Holy Office, but was banished to the villa of the Grand Duke of Tuscany at Trinità dei Monti, which he was to consider as a prison. A week later he was allowed to retire to Siena to the palace of Archbishop Ascanio Piccolomini (a former pupil in Padua), where he was to remain under the orders of the Archbishop, and on no account to leave the house, except to hear mass, without permission from Rome.

He was informed of this decision on July 2, and early on the 6th he shook the dust of Rome from off his feet. He reached Siena on the 9th and was warmly received by Piccolomini and other friends; but kindness could not make him forget that he was a prisoner. As the weary months rolled on, the old man became a little resigned to the situation, and began to occupy

[1] Cf. Bruno and Galileo, *Quarterly Review*, 1878, No. 290, p. 362, a Plutarchian contrast by John Wilson.


Fig. 10.

DISCORSI
E
DIMOSTRAZIONI
MATEMATICHE,
*intorno à due nuoue scienze*
Attenenti alla
MECANICA & i MOVIMENTI LOCALI,
*del Signor*
GALILEO GALILEI LINCEO,
Filosofo e Matematico primario del Serenissimo
Grand Duca di Toscana.
*Con vna Appendice del centro di grauità d'alcuni Solidi.*



IN LEIDA,
Appresso gli Elsevirii. M. D. C. XXXVIII.

Fig. 11.

himself with another of his great works, *Dialoghi delle Nuove Scienze*, the writing of which he spoke of as far back as 1610, in his letter to Vinta.

In November, 1633, thinking the time favourable, the Tuscan Ambassador in Rome began to move for a free pardon, but the Pope was not disposed to go so far, and pretended there would be a difficulty in getting the consent of the Holy Office—a patent evasion, as the decision rested solely with himself. At length, on December 1, 1633, the question of pardon or rather release from personal restraint came before the Congregation, the Pope presiding. It was refused, but Galileo was allowed to retire to his villa at Arcetri, near Florence, where he was to remain till further orders.

## V. Declining Years (1634–42)

### 1. *Dialoghi delle Nuove Scienze* [1]

The Dialogues on the New Sciences ((1) on coherence and resistance to fracture, and (2) on uniform, accelerated, and violent or projectile motions) were begun in Siena in 1633, and were completed by the end of 1634. After the condemnation in 1633, the Holy Office placed Galileo's name on the list of authors whose writings *edita et edenda* were strictly forbidden. Galileo tried to publish at Vienna, only to find that all books printed there must be approved by the Jesuits, amongst whom happened to be Father Scheiner. He tried Olmütz and Prague, but at each place licensing and other difficulties cropped up. Finally he opened direct negotiations with the Elzevirs, and the work was issued from their press at Leyden early in 1638.[2]

The *Dialoghi delle Nuove Scienze* is practically a compendium, with later additions, of his early mechanical work in Pisa and Padua, and rightly did he call it, in his letter to Vinta of May 7, 1610, a new science invented by himself from its very first principles.

'Dynamics', says Lagrange, 'is a science due entirely to the moderns, and Galileo is the one who laid its foundations. Before him philosophers considered the forces which act on bodies in

[1] Cf. Nat. Ed., vol. viii, pp. 12 et seq.; vol. xiv, p. 386; vol. xv, pp. 248, 257, 284 vol. xvi, pp. 59, 72.

[2] See Fig. 11. In order to obviate trouble with the Holy Office, he pretended that the book was pirated from a manuscript copy, which he had given to Comte de Noailles (lately French Ambassador in Rome), to whom it is dedicated.

a state of equilibrium only, and, although they attributed in a vague way the acceleration of falling bodies and the curvilinear movement of projectiles to the constant action of gravity, nobody had yet succeeded in determining the laws of these phenomena. Galileo made the first important steps, and thereby opened a way, new and immense, to the advancement of mechanics as a science.'[1]

In this new dialogue the discussion is carried on by the same speakers as in the Dialogue of 1632. The first two Days are mainly concerned with the resistance of solids to fracture, and the cause of their coherence. Their scientific value lies in the incidental experiments and observations on motion through resisting media. The debate opens with an examination of the current belief that machines built on exactly similar designs, but on different scales, were of strength in proportion to their linear dimensions. It is shown that the larger machine will equal the smaller in all respects, except that it will not be so strong or so resistant to violent actions. After explaining the strength of ropes made of fibrous or filamentous materials, he comes to the cause of coherence of the parts of such things as stones and metals, which do not show a fibrous structure. Presently, Salviati asks what is the nature of the force that prevents a glass or metal rod, suspended from above, being broken by its own weight or by a pull at the free end ? No satisfactory explanation is forthcoming ; but that suggested depends upon nature's so-called repugnance to a vacuum, such as is momentarily produced by the sudden separation of two flat surfaces. This idea is extended, and a cause of coherence is found by considering every body as composed of very minute particles, between any two of which is exerted a similar resistance to separation.

This leads to an experiment for measuring what is called the force of a vacuum. The experiment occasions a remark from Sagredo that his lifting-pump would not work when the water had sunk to the depth of 35 feet below the valve. This story is sometimes told as if Galileo had said jokingly that nature's horror of a vacuum does not extend beyond 35 feet ; but it is plain that the remark was made seriously. He held the then current notion of suction, for he compares the column of water to a rod of metal suspended from its upper end, which may be lengthened till it breaks with its own weight. It is extraordinary that he failed to see how simply this phenomenon could

[1] *Méchanique analytique*, Paris, 1788.

be explained by the weight of the atmosphere, with which he was well acquainted.[1]

We come next to the violent effects of heat and light. It is suggested that, perhaps, heat dissolves bodies by insinuating itself between their minute particles. The effects of lightning are mentioned and experiments with burning-glasses referred to. Then as regards light, Sagredo asks whether its effect does or does not require time. Simplicio is ready with an answer—that the discharge of artillery proves the transmission of light to be instantaneous, to which Sagredo cautiously replies that nothing can be gathered from that observation except that light travels more swiftly than sound; nor can we draw any decisive conclusion from the rising of the sun. Who can assure us that he is not in the horizon before his rays reach our eyes?

We next come to Aristotle's ideas on motion *in pleno* and *in vacuo*, and especially his assertion that bodies fall with velocities proportional to their weights and inversely proportional to the densities of the media through which they are moving. This proposition is examined in a strict scientific method. Heavy bodies of different weights are dropped in air to test the truth of the first part of the statement; and afterwards the motion of bodies rising or falling in liquids is considered; the result being to substitute for Aristotle's assumption that law of the motion of falling bodies which is the foundation of the science of dynamics.

After having discussed the first point and shown that the rate of fall is not proportional to weight, Salviati proceeds to examine the motion of bodies sinking or rising in water and other liquids; and brings forward experimental facts which, viewed in the light of Aristotle's statement, form a mass of contradiction. Putting then this antiquated theory aside, Salviati inquires what is meant by the rising of some bodies in a medium, and shows that only those bodies rise which are lighter than the medium. The rising of an inflated bladder in the air suggests that the atmosphere must have weight. Simplicio's assertion that it is on the contrary the bladder in this case that has levity is trivial, and is immediately disproved. Continuing his line of argument, Salviati points out that the question of rising or falling depends on the gravity of the medium as compared with that of the moving body; further, that when the motion of the body, either upwards or downwards,

[1] Nat. Ed. xi. 12, and xii. 33.

has once commenced, the different media offer different resistances to the motion, the heavier media, such as quicksilver and water, interfering more than air. We are thus led to the bold deduction 'that if the resistance of the media be wholly taken away all matter would descend with the same velocity'.

In the concluding part of this first Day the theory of the pendulum is applied to musical concords and discords, which result from the concurrence or opposition of vibrations of the air striking upon the drum of the ear. These vibrations may be made manifest by rubbing the finger round a glass set in a large vessel of water; 'and, if by pressure the note is suddenly made to rise to the octave above, every one of the undulations, which will be seen regularly spreading round the glass, will suddenly split into two, proving that the vibrations that occasion the octave are double those belonging to the simple note'. Galileo then describes a method he discovered by accident of measuring the length of these waves more accurately than can be done in the agitated water. While scraping a brass plate with an iron chisel, he occasionally produced a hissing or whistling sound, and whenever this occurred he observed the dust on the plate to arrange itself in small parallel streaks equidistant from each other. In repeated experiments he produced different tones by scraping with greater or less briskness, and remarked that the streaks produced by high notes stood closer together than those from low. Among the sounds produced were two, which by comparison with a viol he ascertained to differ by an exact fifth; and measuring the spaces between the streaks he found thirty of one equal to forty-five of the other, which is exactly the proportion of the lengths of strings of the same material which sound a fifth to each other.[1]

Salviati also remarks that if the sounding materials be different, as for instance if it be required to sound an octave to a note on catgut on a wire of the same length, the weight of the wire must be made four times as great, and so on for other intervals.

Salviati: 'I will now show you an experiment from which the eye will derive a similar pleasure. Suspend three balls of lead by cords of different lengths, such that, while the longest makes two oscillations, the shortest makes four, and the medium three. This will occur when the longest string measures 16 of any assumed

[1] This beautiful experiment has been largely used in modern times by Chladni, Savart, and Wheatstone, with very interesting results.

unit (inches, feet, or yards), the medium 9, and the shortest 4. Now, pull these three pendulums aside and let them go at the same instant. You will observe a very curious interplay of the strings, passing each other in various ways, but such that, at the completion of every fourth oscillation of the longest pendulum, all three arrive simultaneously at the same terminus, whence they start afresh to perform the same cycle. This combination of oscillations is precisely that which in music yields the interval of the octave and the intermediate fifth. By changing the lengths of the cords in such ways that their oscillations correspond with agreeable musical intervals, we shall see quite different crossings, but always such that, after a definite time and after a definite number of oscillations all the pendulums will reach the same terminus at the same moment, and then begin again and so on. If, however, the strings are altered so that the oscillations are incommensurable and never complete a series together, or if commensurable complete the series only after a long interval and after a great number of oscillations, then the eye is offended by a disorderly succession of crossing threads, just as the ear is pained by an irregular sequence of air waves.'

The second Day is occupied entirely with an investigation of the strength of beams—an amplification of his researches on the same subject dating back to 1609. Beyond Aristotle's remark that long beams are weak, because they are at once the weight, the lever, and the fulcrum, nothing appears to have been written on the subject before Galileo took it up. The discussion opens with a consideration of the resistance of solid bodies to fracture. This resistance is very great in the case of a direct pull, but is much less in the case of a bending force. Thus, a rod of glass or iron will bear a longitudinal pull of, say, 1,000 lb., while 50 lb. will break it if it be fastened by one end into a wall or other upright support. It is this second kind of strain that is here discussed.

'In order to arrive at the resisting values of prisms and cylinders of the same material, whether alike or not in shape, length, and thickness, I shall take for granted the well-known mechanical principle of the lever, namely, that the force bears to the resistance the inverse ratio of the distances which separate the fulcrum from the force and resistance respectively.'

He assumed as the basis of his inquiry that the forces of cohesion with which a beam resists a cross fracture in any section may all be considered as acting at the centre of gravity of the section, and that it breaks always at the lowest point. An elegant


Plate XLI HALL OF THE GALILEO MUSEUM IN FLORENCE

result deduced from this theory is that the form of a beam, to be equally strong in every part, should be that of a parabolic prism, the vertex of the parabola being the farthest removed from the point of support. As an easy way of drawing the curve for this purpose, he recommends tracing the line in which a heavy flexible string hangs supported from two nails.[1]

The curvature of a beam under any system of strains is a subject into which, before the days of Newton, it was not possible to inquire, and even in the simpler problem considered by Galileo he makes assumptions which require justifying. His theory of beams is erroneous in so far as it takes no account of the equilibrium which must exist between the forces of tension and compression over any cross-section.

In the third Day we find no new physical facts of importance, and much of the time is taken up with theorems and formulae deduced geometrically from the phenomena of uniform and accelerated motion as dealt with in the first Day. The further discussion of that subject, however, leads to a more detailed statement of the principle of inertia; but the definition of uniformly accelerated motion at once introduced a difficulty. Salviati gives the correct description of it as that of a body which moves in such a manner that in equal intervals of time it receives equal increments of velocity.

There follows an interesting application of the results obtained. He examines the times of descent down differently inclined planes, assuming as a postulate that the velocity acquired was the same for all planes of the same height. This fact he had verified by careful experiments, although he was unable at the time to prove it mathematically.[2]

The fourth Day plunges at once into the consideration of the 'properties which belong to a body whose motion is

[1] This curve is not strictly a parabola, it is now called a catenary; but it is plain, from the description of it further on (in the fourth Day), that Galileo was aware of this fact. The catenary resembles the parabola, and, as he justly remarks, the resemblance is all the more striking if the string is so taut that the depth of the lowest point is less than a quarter of the distance between the two extremities.

[2] Viviani relates that, soon after he joined Galileo in 1639, he drew his master's attention to this. The same night, as Galileo lay in bed, sleepless through indisposition, he discovered the necessary mathematical demonstration. It was introduced into the subsequent editions of the Dialogues, sixth Day.

compounded of two other motions, one uniform, and one naturally accelerated. This is the kind of motion seen in a projectile.'

After some preliminary instruction in the properties of the parabola, Salviati returns to the subject of projectiles, and lays down the law of the independence of the horizontal and the vertical motions. A body projected horizontally would—but for its weight and external impediments—continue to move in a straight line ; and Salviati contends that, as the effects of gravity acting by itself would be entirely downwards, gravity acting on the projected body can neither increase nor diminish the rate at which it travels horizontally. Therefore, whatever be the shape of the path or the direction of motion at any moment, the distance travelled horizontally may be taken as a measure of the time that has elapsed since motion began. He proves that on this assumption the path described has geometrical properties which identify it with the curve known as the parabola. His demonstration is essentially that now given in works on elementary dynamics.

After demonstrating the parabolic nature of the path, he inquires into certain points of interest with regard to it, and gives proofs of many of the elementary propositions which in modern text-books are associated with parabolic motion. He also draws up a table giving the position and dimensions of the parabola described with any given direction of projection, finding by this means what he would have been unable to give a strict mathematical proof of—that the range on a horizontal plane is greatest when the angle of elevation is 45°.[1]

## 2. *The Laws of Motion*

No sooner was the manuscript of these dialogues out of his hands (summer of 1636) than Galileo occupied himself with new projects. 'If I live,' he wrote on July 15, 1636, to Berneggar of Strasburg, 'I intend to put in order a series of natural

[1] 'In solving the problems of falling bodies and of projectiles, Galileo was essentially applying the principles of the Differential or Fluxional or Indivisible Calculus. If pure mathematics had attracted him as strongly as its application to physics, he would have thought these problems out, and would have founded the Fluxional Calculus, which is the glory of Newton and of Leibnitz.' Professor Jack, in *Nature*, vol. xxi, p. 58.

and mathematical problems which I think will be as curious as they are novel.' These were left unfinished, and now form the fifth and sixth Days, which were added to later editions by Viviani after Galileo's death.[1] The fragment of the fifth Day is on the subject of Euclid's definition of ratio (Book V, props. 5 and 7) and was intended to follow the first proposition on equable motion in the third Day's debate. The sixth Day contains his investigations on the force of percussion, on which he was employed at the time of his death.

'In the last days of his life', says Viviani, 'and amid much physical suffering, his mind was constantly occupied with mechanical and mathematical problems. He had the idea of composing two other dialogues to be added to the four already published. In the first he intended to give many new demonstrations and reflections on various passages in the first four dialogues, and the solution of many problems in Aristotle's physics. In the second he proposed to discuss, treating it geometrically, an entirely new science, *viz.* the wondrous force of percussion, which he claimed to have discovered and which, he said, exceeded by a long way his speculations on the same subject formerly published' (Nat. Ed., vol. xix, Part ii).

In these admirable dialogues Galileo does not formulate in definite laws the interdependence of force and motion. This was done for the first time by Newton at the beginning of his *Principia* (1687), and hence they are rightly called 'Newton's Laws of Motion'; but in justice to Galileo it must be admitted that he not only prepared the way for Newton, but supplied him with much of his materials. Thus, the first law—that a body will continue in a state of rest, or of uniform motion in a straight line, until it is compelled to change its state by some force impressed upon it—is a generalization of Galileo's theory of uniform motion. Since all the motions that we see taking place on the surface of the earth soon come to an end, we are led to suppose that continuous movements, such, for instance, as those of the celestial bodies, can only be maintained by a perpetual consumption and a perpetual application of force, and hence it was inferred that rest is the natural condition of things. We make, then, a great advance when we comprehend that a body is equally indifferent to motion as to rest, and that it equally perseveres in either state until disturbing forces are applied.

[1] Cf. Nat. Ed., vol. viii, pp. 321–62.

The second law—that every change of motion is in proportion to the force that makes the change, and in the direction of that straight line in which the disturbing force is impressed—is involved in Galileo's theory of projectiles. Before his time it was a commonly received axiom that a body could not be affected by more than one force at a time, and it was therefore supposed that a cannon-ball, or other projectile, moves forward in a straight line until the force which impelled it is exhausted, when it falls vertically to the ground.

The establishment of this principle of the composition of forces supplied a conclusive answer to the most formidable of the arguments against the rotation of the earth, and, accordingly, we find it in the second Day of the Dialogue of 1632. The distinction between mass and weight was, however, not noticed, and, consequently, Galileo failed to grasp the fact that acceleration might be made a means of measuring the magnitude of the force producing the motion. How far he was from this discovery may be gathered from a remark by Salviati, incidental to the main argument, to the effect that when different bodies are falling freely towards the earth's centre, 'the difference of their gravities has nothing to do with their velocities.'

Of the third of the laws of motion—that action and reaction are always equal and opposite—we find traces in many of Galileo's researches, as in his theory of the inclined plane, and in his definition of momentum. It is also adumbrated in his 'Della Scienza Meccanica' (1594), and in his latest ideas on percussion.

His services were little less conspicuous in the statical than in the dynamical division of mechanics. He gave the first direct and entirely satisfactory demonstration of equilibrium on an inclined plane. In order to demonstrate this he imagined the weight and the resistance to be applied to the ends of a bent lever whose arms were equal to the vertical and slant sides of the plane; then reducing the lever to a straight one it was easy to prove that the forces *in equilibrio* on the plane were also *in equilibrio* on the lever, and were to one another as the length to the height of the plane. By establishing the theory of 'Virtual velocities', he laid down the fundamental principle which in the opinion of Lagrange contains the general expression of the laws of equilibrium; while as regards that still obscure subject, molecular cohesion, he brought it for the first time within the range of mechanical theory.

### 3. *On the Moon's Librations* [1]

Just before his sight began to fail,[2] Galileo made his last astronomical discovery, which is now known as the moon's librations. This discovery was announced in letters to Fulgenzio Micanzio (November 7, 1637) and Alfonso Antonini (February 20, 1638). To the former he writes:

'I have observed a marvellous appearance on the surface of the moon. Though she has been looked at such millions of times by such millions of men, I do not find that any have observed the slightest alteration in her surface; but that exactly the same side has always been supposed to be presented to our eyes. Now I find that such is not the case, but that she changes her aspect, as one who, having his full face turned towards us, should move it sideways, first to the right and then to the left; or should raise and then lower it; or, lastly, should incline it first to the right shoulder, then to the left. All these changes I see in the moon; and the large, anciently-known spots which are seen on her face will help to make evident the truth of what I say. Add to these a further marvel, which is that these three mutations have their several periods—the first daily, the second monthly, the third yearly.'

Galileo was not long in detecting the causes of the apparent libratory or rocking movement. The diurnal or parallactic libration he saw was occasioned by our distance as spectators from the centre of the earth, which is also the centre of the moon's revolution. In consequence of this, as the moon rises we get an additional view of the lower part and lose sight of that portion of the upper part which was visible while she was low in the horizon. The causes of the other motions are not so easily explained, nor is it certain that Galileo himself understood them; his conjecture of a connexion with the tides is certainly wide of the mark.

The moon in revolving round the earth spins once on her axis, so turning the same side always towards the earth's centre. But this familiar truth is only approximate. The speed of rotation is uniform; but the speed of revolution in her orbit is not so, because that orbit is not a circle but an ellipse, in which (as is always the case with elliptic motion) the moving body travels faster while

[1] Cf. Nat. Ed., vol. xvii, pp. 212, 291.

[2] Early in 1636 his sight began to fail. By the end of June, 1637, the sight of the right eye was gone, and early in December following he became totally blind.

near the centre of attraction than when farther away. The result is that we see alternately a little round the eastern edge, and a fortnight later a little round the western.

The two librations, due to independent causes, have approximately the same period—about one month. Their effects, however,

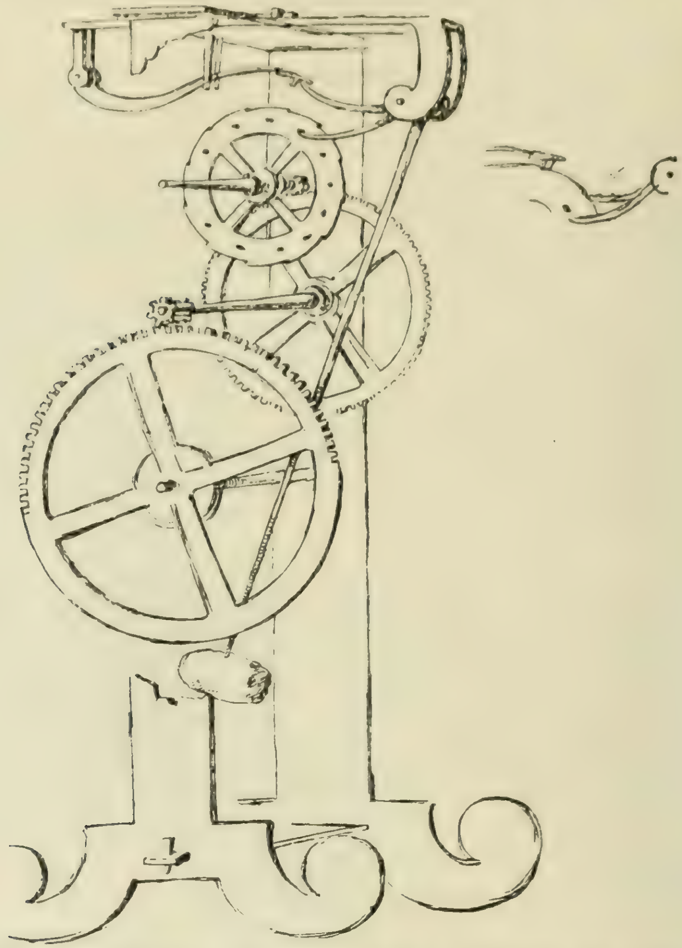


FIG. 12. FACSIMILE OF DESIGN FOR A PENDULUM CLOCK.
Drawn by Vincenzio Galilei from his father's dictation.

vary according to the changing position of the earth in its orbit: and any particular phase of the libration is more nearly reproduced after twelve months than after one. Galileo was, therefore, justified in suggesting an annual period, although it is not customary at the present day to associate the annual period with any very distinct librations.

### 4. *Application of the Pendulum to Clocks* [1]

A few months before his mortal illness Galileo once more gave proofs of his mechanical genius. It has been remarked in the progress of science and scientific invention that the steps, which on looking back seem the easiest to make, are often those which are the longest delayed. The application of the pendulum to clocks is an instance of this. We have seen that Galileo was early convinced of the value of the pendulum as a measurer of time, and that as far back as 1582–3 he used it in the Pulsilogia; yet fifty-five years later, although constantly using it meanwhile, he had not devised a more practicable application than that described in his letter of June 6, 1637, to Lorenzo Realio, and in his 'Astronomical Operations' of 1637–8 for finding the longitude at sea.

'I make use of a heavy pendulum of brass or copper, in the shape of a sector of twelve or fifteen degrees, the radius of which may be two or three palms (the greater it is the less trouble in attending it). This sector I make thickest in the middle radius, tapering gradually towards the edges, where I terminate it in a tolerably sharp line, to obviate as much as possible the resistance of the air, which is the main cause of its retardation. This sector is pierced at the centre, through which is passed an iron bar shaped like those on which steelyards hang, terminated below in an angle or wedge which rests on two bronze supports. If the sector (when accurately balanced) be removed several degrees from the perpendicular, it will continue a to-and-fro motion through a very great number of oscillations before coming to rest; and in order that it may continue its oscillations as long as it is wanted, the attendant must occasionally give it a push so as to carry it back to large oscillations.

'Now to save the fatigue of continually counting the oscillations, this is a convenient contrivance,—a small delicate needle extends from the middle of the sector which in passing strikes a rod hung at one end. The lower end of this rod rests on the teeth of a horizontal wheel as light as paper. The teeth are cut like those of a saw. The rod striking against the perpendicular side of a tooth moves it, but when returning it slips over the oblique side of the next tooth and falls at its foot, so that the motion of the wheel will be in one direction only. By counting the teeth you may see at will the number passed, and, consequently, the number of oscillations or periods of time which you wish to measure. You may also fit to the axis of the wheel a second, with

[1] Cf. Nat. Ed., vol. viii, p. 451; vol. xvii, pp. 96, 212; vol. xix, pp. 647–59.

a smaller number of teeth and in gear with a third wheel having a greater number of teeth, and so on.'

It was chiefly because of the inadequacy of this method that the negotiations with the States-General were finally broken off. Now, in the second half of 1641, it occurred to Galileo that the problem could be solved by adding the pendulum to the ordinary clock as a regulator of its movements. He explained his idea to his son, Vincenzio, who made a drawing (of which we reproduce a facsimile, Fig. 12) from his father's direction. Before the plan could be tried Galileo fell ill, and died January 8, 1642. The matter was laid aside, but seven years after his father's death Vincenzio resumed it, and was engaged in constructing what would have been the first pendulum clock, when he, too, fell ill and died, May 16, 1649.[1]

[1] A working model of this clock, inscribed ' Eustachio Porcellotti costruito a Firenze l'anno 1883 ', is in the Science Museum, South Kensington, and keeps very good time.

# THE HISTORY OF ANATOMICAL INJECTIONS

By F. J. Cole

'The purpose of injections,' says Robin with admirable brevity, 'is to make known to us the absolute vascularity of the tissues'; and the practice of the method, to quote Lacauchie, 'founded an hundred reputations, and filled the museums with beautiful preparations'. The history of a scientific method such as this, established alike by its aims and results, may well be traced in some detail.

It will not surprise many to learn that the scope of this inquiry cannot be kept within the terms of Robin's definition. No method of scientific research long retains its original character. The further we trace it back the more it changes, until the question when, how, and with whom it originated may indeed be debated but not resolved. Thus the earlier injections were of a general or random character, and were used to fill the larger cavities of the body, such as the bladder from the penis, or to explore the solid inscrutable bulk of tumours. Afterwards specific injections into blood-vessels were attempted, but even then the endeavour was to test or illustrate the doctrine of the circulation, or to demonstrate the local distribution of certain vessels.

The reception of the injection method did not quite follow the traditional course. The results obtained by it were too obvious and striking to be either questioned or ignored. It passed rapidly into general use, and quickly reached its point of maximum utility. For a time it monopolized attention, and its importance was so grossly exaggerated as to countenance the belief that all problems of anatomy and physiology might be solved by its means.[1] In 1727 the French Academy of Science could find no more worthy

[1] The method is honoured by a reference in Gibbon's *Decline and Fall* (first edition, 1788, vol. v, p. 429). He says: 'A superstitious reverence for the dead confined both the Greeks and the Arabians to the dissection of Apes and Quadrupeds; the more solid and visible parts were known in the time of Galen, and the finer scrutiny of the human frame was reserved for the Microscope and the Injections of modern artists.'

successor to Sir Isaac Newton than Frederik Ruysch—the most famous injector of his time. How unsound contemporary judgement may be could not have a more significant confirmation than the verdict of posterity on the work of these two men—a verdict which exemplifies the practical wisdom of Boyle's remark that 'we are not near so competent judges of wisdom as we are of justice and veracity'. The frenzy for injections finally exhausted itself, and as a means of research the method may almost be said to have served its purpose, and to have passed into the shadow of history. In stating this, however, certain modern developments must not be ignored. In 1886 Camillo Golgi introduced the intra-vitam method of fixing animal tissues by a vascular injection of the fixative, and in 1900 Flint studied the circulation in the living embryo by throwing an injection into the blood-stream.

There are manifestly several important questions which the injection method is peculiarly adapted to elucidate. The circulation of the blood can be demonstrated beyond question by an injection experiment; the existence of foetal and uterine portions in the placenta is establishable only with the assistance of the syringe; the connexion of the vas deferens, epididymis, and testis is made beautifully clear by mercury injections; and the independence of the blood vascular and lymphatic systems rests upon a series of careful injections. Again, certain negative results may claim attention. Descartes' speculation that nerve fibres were valvular tubes transmitting fluid contents was finally disposed of by injection experiments, in spite of the fact that at first such support was in some cases claimed for it. On the other hand, erroneous views were undoubtedly suggested and maintained by the injections of the earlier anatomists. Of these lapses mercury was the most frequent cause, and on that account it is now rarely if ever used. Even when a mercury injection is successful, the least abrasion results in an extensive bleeding of the mercury, and preparations so delicate were justly considered to have but a doubtful value. With other injection masses extravasation effects frequently resulted in serious error and controversy among anatomists, and tended to bring the method into disrepute. The disputes between the Hunters and the Monros owed something to this cause, and extravasations into minute extra-vascular spaces of a tubular character explain the 'capillicules' of some of the later French injectors.

The frequent use of the word 'art' indicates an appreciation of the beauty of the injected specimen by the earlier anatomists, and it is easy to picture their wonder as smaller and yet smaller vessels were disclosed. What, therefore, must their delight have been when, the unaided eye being able to see no more, the microscope revealed the amazing spectacle of the blood capillaries in all their profusion of number and form. We can scarcely blame them if they abandoned Science for display, and injections were undertaken merely because the result was picturesque. Pole says: 'The veins in the kidney of a cat run very superficial, and branch out in a manner peculiarly beautiful, which is the only inducement to make this preparation.' And again, an injected and corroded kidney of the horse 'makes a noble and beautiful preparation'.

In this article it is not proposed to refer other than briefly to the injection of living animals, or to the transfusion experiments which greatly occupied the attention of the Royal Society and the French Academy of Science in the second half of the seventeenth century. The history and literature of this subject has already been exhaustively dealt with, but it does concern also the anatomical application of the injection method, and to that extent must receive attention. Although certain transfusion and intravitam injections had been attempted before the doctrine of the circulation was established in 1628, there can be no question that Harvey's work was the fundamental and determining influence in these early injection experiments. Before 1650, however, the new doctrine, though indeed accepted, had not succeeded in energizing contemporary investigation, and it was only from this latter date onwards that a further advance is to be noted. The period of injection lies between 1650 and 1750, after which time interest in injections gradually declined as it was found that the new method was unable to realize the great expectations of its professors. Ruysch's activities are comprised between the dates 1665 and 1728, and hence coincide almost exactly with the middle of the injection period. The first injections, however, were not anatomical, but were undertaken for physiological or medical reasons. The object was to transfer blood from one living animal to another, and to test the effect of injecting various liquids, drugs, or air into the vessels of the living body. Great hopes were based on these haphazard and ruthless efforts. In 1667, and again in

1670, the French Academy conducted numerous injection experiments in the belief that the reinvigoration and even the rejuvenescence of mankind would be thereby accomplished, but they were quick to acknowledge the vanity and futility of their hopes. The failure, however, was not absolute, for it was soon perceived that the humbler but still important requirements of anatomy could be promoted by the new method, and in a short time the success of the anatomical injection provided an ample compensation for the failure of the physiological.

Before 1650 there exist only a few references to injection experiments in the literature of Biology. The idea was in the background, but its exploitation was delayed. In this early work we should expect the seeker to precede the syringe, and thus we find Aretaeus the Cappadocian,[1] who lived in the second or third century of the Christian era, demonstrating that 'one may pass a plate of metal from the vena cava connected with the heart to that by the spine, and from the spine through the liver to the heart; for it is the same passage leading upwards'. Galen[2] goes a step further, and studied the distribution of the cerebral vessels by inflating them with air through a tube. Thus the blood channels were distended, and acquired the relief necessary to enable them to be followed the more easily. According to Michele Medici, Alessandra Giliani of Persiceto, who died in 1326, filled the blood-vessels with liquids of different colours, which thickened and hardened as soon as they were injected, but did not decompose. This statement, however, lacks confirmation. The first reference to injections after the invention of printing occurs in the commentary of Jacobus Berengarius published in 1521.[3] He employed a syringe and injected the renal veins with warm water—'per syringam, aqua calida plenam'. Massa (1536)[4] inflated the kidney by forcing air into the renal vein, and Stephanus

[1] Aretaeus Cappadox, *De causis et signis acutorum et diuturnorum morborum*, Venice, 1552, 4to. *The extant works of Aretaeus the Cappadocian*, F. Adams, London, 1856, p. 280, 8vo.

[2] Claudius Galenus (*c.* 130–200), *De Anatomicis Administrationibus*, lib. ix, cap. 2. Ed. C. G. Kühn, Leipzig, 1821–33, 8vo.

[3] Giacomo Berengario da Carpi (1470–1530), *Commentaria cum amplissimis additionibus super anatomiam Mundini*, Bologna, 1521, 4to.

[4] Nicolaus Massa (ob. 1569), *Anatomiae Liber Introductorius, seu dissectionis corporis humani*, Venice, 1536, 4to.

(1545) [1] devised a pump to inflate the vessels with air—thus making their distribution more conspicuous to the unaided eye. Sylvius,[2] in his last work published shortly after his death, 'the work of his old age', states that the blood-vessels are better seen if they are inflated with air, or when a coloured liquid is introduced into them by means of little tubes. He used variously coloured fluids such as saffron and wines, but rejects the injection method because the liquid escapes when the vessels are cut and the preparation is thereby spoilt. In 1556 the Portuguese physician Amatus Lusitanus [3] filled the vessels with a liquid by means of a siphon, or forced it in through a tube by air pressure supplied from the mouth.

Eustachius [4] is undoubtedly one of the pioneers of the modern injection method, and is credited by later authors, such as Portal and Milne-Edwards, with having practised more elaborate injections than his writings justify. In his work on the structure of the kidney he describes the following procedure. The kidney was kept warm by applying to it a sponge soaked in hot water. Tubes were inserted into the renal artery and vein, and spirit or water was injected into the vessels by the force of the breath. The fluids entered the kidney, extended through its substance, and passed ultimately into the pelvis and ureter. In the converse experiment of injecting the ureter it was found that the injection passed into the tissue of the kidney and reached the arteries and veins. Again, the renal artery was inflated, and air found its way into the ureter. The erroneous belief in a direct connexion between the arteries and the uriniferous tubules, based as it was on injection experiments, survived for a long time, and was taught in the anatomical schools throughout the eighteenth century. It was not until Bowman published his work on the kidney in 1842 that the correct relation of the renal arteries and veins to the glandular tubules was first completely demonstrated. Eustachius also followed the course of the vessels by the tedious process of

[1] Charles Estienne (1504–64), *De dissectione partium corporis humani*, Paris, 1545, fol.

[2] Jacques Dubois (1478–1555), *In Hippocratis et Galeni physiologiae partem anatomicam isagoge*, Paris, 1555, fol.

[3] Johannes Rodriguez da Castello Bianco (1511–68), *Curationum Medicinalium Centuriae* 7, Basle, 1556, Cent. 4 fol.

[4] Bartolommeo Eustacchi (1520–74), *De Renum Structura*. Venice. 1563, 4to; *Tabulae Anatomicae*, Rome, 1714, fol. Text by J. M. Lancisi.

excarnation, in which, unlike later anatomists, he does not appear to have used any injection. Boerhaave is of opinion that Eustachius must have known of a more perfect method of investigation than inflation with air, because of the great exactness with which he traced the vessels, and von Haller considers that plates 13 and 27 of the *Tabulae*, published long after the death of Eustachius, represent the vessels so skilfully that it is hardly credible they could have been prepared without the assistance of fluid injections, unless, as Boerhaave adds, Eustachius brought to bear upon the study of anatomy an 'application and exactness more than human'. In recommending the injection of liquids he was certainly in advance of a time which favoured the distension of the vessels with air rather than filling them with a more solid medium. Thus Laurentius, in 1593,[1] ascertained that there was a connexion between the umbilical vein and the portal vein and inferior vena cava of the foetus by inflating the first named with a 'hollow bugle', and Crooke, in 1615,[2] refers to the necessity of having 'reeds, quils, glasse-trunkes or hollow bugles to blow up the parts'.

At about this time the possibility of diverting blood from the vessels of one living animal into those of another was first conceived and practised. The earliest writer to mention transfusion experiments was Magnus Pegel in 1604, and others are Andreas Libavius (1546–1616) in 1615 and Johannes Colle (ob. 1631) in 1628. It is to be noted that these works were published before Harvey's treatise on the circulation of the blood.

It is appropriate that the most important of the early injection experiments should have been made by Harvey himself—'that ocular philosopher and singular discloser of truth'. His treatise on the circulation, however, is silent on the matter of injections. In a letter addressed to Paul Marquard Slegel, dated March 26, 1651, and published later by Sir George Ent,[3] he returns to the criticisms of Riolan on the doctrine of the circulation, and adds the following remarkable piece of evidence:

'It may be well here to relate an experiment which I lately tried in the presence of several of my colleagues and from the

[1] André du Laurens (1558–1609), *Historia Anatomica Humani Corporis*, Leyden, 1593, 8vo.

[2] Helkiah Crooke (1576–1635), *Microcosmographia*, London, 1615, fol.

[3] Sir George Ent (1604–89), *Opera omnia Medico-Physica*, Leyden, 1687, 8vo.

cogency of which there is no means of escape. . . . Having tied the pulmonary artery, the pulmonary veins and the aorta, in the body of a man who had been hanged, and then opened the left ventricle of the heart, we passed a tube through the vena cava into the right ventricle of the heart, and having, at the same time, attached an ox's bladder to the tube, in the same way as a clyster-bag is usually made, we filled it nearly full of warm water, and forcibly injected the fluid into the heart, so that the greater part of a pound of water was thrown into the right auricle and ventricle. The result was, that the right ventricle and auricle were enormously distended, but not a drop of water or of blood made its escape through the orifice in the left ventricle. The ligatures having been undone, the same tube was passed into the pulmonary artery, and a tight ligature having been put round it to prevent any reflux into the right ventricle, the water in the bladder was now pushed towards the lungs, upon which a torrent of the fluid mixed with a quantity of blood, immediately gushed forth from the perforation in the left ventricle ; so that a quantity of water, equal to that which was pressed from the bladder into the lungs at each effort, instantly escaped by the perforation mentioned. You may try this experiment as often as you please ; the result you will still find to be as I have stated it.'

Priority of publication, however, for this experiment belongs to others. Marchettis, for example, who supported Harvey as against Riolan, in the presence of Thomas Bartholin in 1652, demonstrated that liquid injected into the arteries emerged by the veins.[1]

Passing over Riolan,[2] who practised inflation of the vessels for demonstration purposes, and Lyser,[3] who published the first general treatise on anatomical methods, in which a blowpipe is mentioned for inflating with air or filling cavities with water, but in which injections are not considered, the next contribution to the subject is by Glisson.[4] He injects with a tube to which a bladder containing the medium is attached. The tube is pushed into the vessel and tied, the bladder filled with the injection and bound to the tube, and the fluid forced into the vessels by compressing the bladder at first gently and then more firmly. The

[1] Domenico de Marchettis (1626–88), *Anatomia, cui responsiones ad Riolanum . . . additae sunt*, Padua, 1652, 4to.

[2] Jean Riolan, *fil.* (1577–1657), *Opuscula anatomica varia et nova*, Paris, 1652, 12mo.

[3] Michael Lyser (1627–60), *Culter Anatomicus*, Copenhagen, 1653, 8vo.

[4] Francis Glisson (1597–1677), *Anatomia Hepatis*, London, 1654, 8vo.

media employed are hot water by itself or mixed with milk, water coloured with saffron, and ink. A figure is given of his apparatus, but it is of little assistance in following the description.

We now come to 'that miracle of a youth—Christopher Wren', whose experiments have been described by Boyle [1] and Oldenberg.[2] According to the Philosophical Transactions of 1665, Wren (1632–1723) first suggested to Boyle at Oxford not later than 1659 [3] to ligature the veins of a living animal, open them on the side of the ligature nearest the heart, and inject with 'slender syringes or quills fastened to bladders'. He recommended as the subject 'pretty big and lean dogs'. This operation appears to have been frequently practised in Oxford, and also in London before the Royal Society. 'And they hope likewise, that beside the *medical* uses, that may be made of this *invention*, it may also serve for *anatomical* purposes, by filling, after this way, the vessels of an animal as full, as they can hold, and by exceedingly distending them, discover *new* vessels.' 'To *Oxford*, and in it, to Dr. *Christopher Wren*, this invention is due.' Wren, then a youth of twenty-four, injected wine and ale into the blood of a living dog by one of the veins, and noted that the animal became extremely drunk. The experiment he takes 'to be of great concernment and what will give great light to the theory and practice of physic', and Sprat refers to it as that 'noble anatomical experiment of injecting liquors into the veins of animals'. We may now complete what remains to be said on the history of physiological injections, which we have already seen were known to Pegel and Libavius long before Wren's first experiment in 1656. According to J. M. Verdries, a large ox at once died when Wepfer injected air into its jugular vein, and Major [4] asserts that transfusion experiments on dogs were practised by Hans Jurge de Wahrensdorf in 1642. Elsholz [5] injected medicines into the veins of man and dogs, and

[1] Hon. Robert Boyle (1627–91), *Of the Usefulnesse of Naturall Philosophy*, Oxford, 1663, 4to, pp. 62–5.

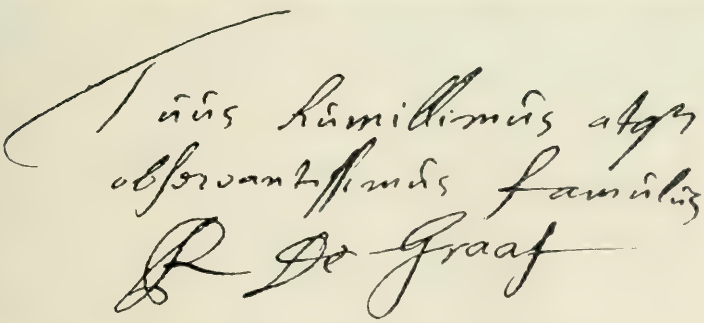
[2] Henry Oldenberg (1615–77), 'Account of the Rise and Attempts of a way to convey liquors immediately into the mass of blood,' *Philosophical Transactions*, London, 1665, 4to, vol. i, p. 128.

[3] An error. Wren's first experiment was undertaken in 1656.

[4] Johann Daniel Major (1634–93), *Prodromus inventae a se Chirurgiae infusoriae*, Leipzig, 1664, 8vo.

[5] Johann Siegesmund Elsholz (1623–88), *Clysmatica Nova*, Berlin, 1665, 12mo.

PLATE XLII. REINIER DE GRAAF




From the first edition of the *De Usu Siphonis* (1668). Signature from a MS. in the possession of the Royal Society (Photograph supplied by Dr. Charles Singer).

also practised transfusion of the blood. Lower (1666) [1] was the first to undertake transfusion of blood from an artery of one animal into the vein of another, and in the following year Jean Denys performed the same operation on man—'a circumstance of great exultation to the French'. Boerhaave, in commenting on this result, says: 'the experiment was soon received with great applause both through France and England, and great things were expected from it in the cure of diseases and the recovery of youth . . . but in a little time all these expectations disappeared, and the experiment was prohibited to be made on men by the public law'. In a letter to N. Steno, dated 1667, but not published at the time, Francesco Redi (1626–98) mentions that he had instantly killed two dogs and a hare by injecting air into the veins, but a sheep and two foxes died more slowly. Finally Clarke (1668) [2] states that he had been engaged for many years in injecting various liquids into the blood of living animals, but he had grave doubts as to its utility except for anatomical purposes. The minutes of the Royal Society for May 28, 1660, record that 'Dr. Clarke was intreated to bring in the experiment of injections into the veins'—which doubtless refers to the physiological experiment.

From Glisson up to the time of de Graaf sufficient progress was made in the methods of injection to justify the statement that de Graaf simply collated and fixed the knowledge of his time, but added to it little that was new. Wepfer [3] investigated the vascular supply of the brain by means of injections of saffron water, and gives the first correct description of the course and branching of the carotid artery and of the vessels of the brain membranes. Important results were achieved by Malpighi.[4] He recommends examining the vessels of the lungs under the powerful illumination of the rays of the sun. If this should prove inadequate

[1] Richard Lower (1631–91), 'The success of the experiment of transfusing the blood of one animal into another', *Philosophical Transactions*, London, 1666, 4to, vol. i, p. 352.

[2] Timothy Clarke (ob. 1672), 'Some anatomical inventions and observations, particularly relative to the origin of the injection into veins, the transfusion of blood, and the parts of generation', *Philosophical Transactions*, London, 1668, 4to, vol. iii, p. 672.

[3] Johann Jakob Wepfer (1620–95), *Observationes Anatomicae*, Schaffhausen, 1658, 8vo.

[4] Marcello Malpighi (1628–94), *De Pulmonibus*, Bologna, 1661, fol.

air may be driven into the main trunk of the pulmonary artery, when the vessels will swell up and appear in all their ramifications as if they were carved with a chisel. Even the smallest branches are dilated and stretched out like the branches of a tree. Or if a still more refined method be necessary, mercury can be syringed into the same vessel, whereupon all its branches up to the finest twigs will assume a beautiful silver colour. But the doubt which still, he says, tortured his mind, which his injection of liquids of different colours did not resolve, was whether the arteries and veins were discontinuous, and only connected up indirectly by a spongy parenchyma, or whether the junction was effected directly by microscopic vessels. He repeats Harvey's experiment, and finds that water or a black liquid syringed into the pulmonary artery emerges by the pulmonary vein. Sometimes also it emerges by the trachea. He is not misled by this surprising result, but realizes that injections may extravasate and give rise to erroneous conclusions. Subsequently he saw the blood capillaries in the lung of the frog, but injections played no part in that memorable discovery. A few years later, in 1666,[1] Malpighi is investigating the structure of the kidney by injection methods. He used ink, urine coloured with ink, a black or other coloured liquid, and a black liquid mixed with spirits of wine. A coloured fluid injected with a syringe into the renal artery reaches the smallest branches of the artery and the 'internal glands' (since called Malpighian bodies), so as to produce the appearance of a 'beautiful tree loaded with apples'. He was not able to inject these bodies from the veins, nor could the uriniferous tubules be injected either from arteries or veins. He regards the uriniferous tubule as the excretory duct of the Malpighian body, but did not succeed in actually demonstrating the connexion between them.

Bellini, who was only nineteen years of age when he produced his remarkable work on the structure of the kidney,[2] is said to have used an injection medium which melted with heat. He mentions mercury, which he appears to have employed for injection purposes, and talks of artificially distinguishing the vessels by injecting some coloured liquid into the renal artery and vein, but does not state the nature of the medium or the colours selected.

[1] Marcello Malpighi, *Exercitatio Anatomica de Renibus*, Bologna, 1666, 4to.

[2] Lorenzo Bellini (1643–1704), *De Structura Renum*, Florence, 1662, 4to.

Robert Boyle [1] is responsible for a striking contribution to the technique of injection. He says:

'And perhaps there may be some way to keep the arteries and the veins too, when they are empty'd of blood, plump, and unapt to shrink overmuch, by filling them betimes with some such substance, as, though fluid enough when it is injected to run into the branches of the vessels, will afterwards quickly grow hard. Such may be the liquid plaister of burnt Alabaster, formerly mention'd, or ising-glass steeped two days in water, and then boild up, till a drop of it in the cold will readily turn into a still gelly. Or else Saccarum Saturni, which, if it be dissolv'd often enough in spirit of vinegar, and the liquor be each time drawn off again, we have observ'd to be apt to melt with the least heat, and afterwards to grow quickly into a somewhat brittle consistence again.'

This is the first unequivocal mention of solidifying injection media, and it is noteworthy that two of them, plaster and gelatin, are in use at the present day, and also that Boyle does not suggest the addition of colour—an improvement with which he can hardly have been unfamiliar. Whether he ever carried his suggestions into practice is doubtful. It is true that Grew in 1681 asserts that Boyle was the first to use wax as an injection mass, but Boyle himself says, 'but I must not insist on these fancies'; and again in 1659, when commenting on the anatomical methods of de Bills, 'most of the ways I proposed to myself were as yet little more than bare designs'. Saccarum Saturni, it may be mentioned, is acetate or sugar of lead, the preparation of which by the old method is given by James.[2] It has a waxy consistency.

In his work on the anatomy of the brain, Willis [3] brings the new method into useful practice. The vessels of the brain, he says, are 'seen better and more distinctly, if you first squirt into the carotidick artery some black liquor'. He injects the rete mirabile of ruminants from the carotid with ink, and finds that the injection reaches the carotid of the other side. In another passage his instruments are mentioned, as follows:

'Let the carotidick arteries be laid bare on either side of the *cervix* or the hinder part of the head, so that their little tubes or

[1] Hon. Robert Boyle (1627–91), *Of the Usefulnesse of Naturall Philosophy*, Oxford, 1663, 4to.

[2] Robert James (1705–76), *A Medicinal Dictionary*, London, 1745, fol., vol. iii, art. Plumbum.

[3] Thomas Willis (1621–75), *Cerebri Anatome*, London, 1664, 4to.

pipes, about half an inch long, may be exhibited together to the sight; then let a dyed liquor, and contained in a large squirt or pipe, be injected upwards in the trunk of one side, after once or twice injecting, you shall see the tincture or dyed liquor to descend from the other side by the trunk of the opposite artery; yea, if the same be more copiously injected towards the head, from thence returning through the artery of the opposite side, it will go thorow below the *Praecordia*, even to the lower region of the body; when in the meantime, little or nothing of the same tincture is carried thorow the outward and greater jugular veins. Then the head being opened, all the arteries, before the entrance of the head, and the veins of the same band with them, will be imbued with the colour of the same injected liquor.'

Some years later, in 1672,[1] Willis was the first to inject an Invertebrate. In an involved passage describing the heart and gills of the lobster, he mentions injecting a 'black liquor' into the heart, from which it passed to the gills (!). He also injected the same liquid into the afferent branchial artery of a fish, and noted that it circulated through the gill and finally reached the dorsal aorta. Notwithstanding this significant result his ideas on the circulation of the blood in fishes are confused and difficult to follow. In 1675 [2] Willis investigates the structure of the lungs by means of injections of the arteries, veins, and bronchi. He notes how these three factors are associated in the lung tissue, and points out how impossible it is to grasp their relations, or make a representation of them, without injections. He recommends 'quicksilver, hot and flowing gypsum, wax mingled and made liquid with oyl of turpentine, or some such matter'. No colours are mentioned, which is again extraordinary, seeing that he was writing after Swammerdam, with whose work he must have been acquainted.

On March 27, 1667, Pecquet [3] described a supposed connexion between the thoracic duct, which he had rediscovered in 1651, and the renal vein in Man by inflating the duct with air through a quill. In this work he was assisted by Louis Gayant, one of

[1] T. Willis, *De Anima Brutorum*, London, 1672, 8vo. Also editions at Oxford and Amsterdam bearing same date.

[2] T. Willis, *Pharmaceutice Rationalis*, 2, Oxford, 1675, 4to.

[3] Jean Pecquet (1622–74), 'A new Discovery of the communication of the ductus thoracicus with the emulgent vein', *Philosophical Transactions*, London, 1667, 4to, vol. ii, p. 461. Translation of a letter which appeared in the *Journal des Sçavans*, 1667, 8vo., and elsewhere.

the Parisian comparative anatomists. On February 8, 1672, Pecquet [1] professes to have discovered by injections a direct connexion between the thoracic duct and the postcaval vein. He forced into the thoracic duct with a syringe hot milk or a substance which was fluid when thrown in hot but solidified on cooling. He gives no indication as to what this substance was, but states that the method succeeded 'in part'. Before the operation, the body was warmed by injecting nearly boiling milk. As regards publication, Pecquet slightly anticipates Swammerdam's 'invention' of a solidifying injection medium, and was himself anticipated by Boyle. Walter Needham, who criticizes Pecquet's paper in the *Philosophical Transactions* for 1672, and points out his mistakes, refers to the 'coagulating injection', but does not appear to have been greatly impressed by it.

A year before the publication of de Graaf's tract, 'Theodorus Aldes' [2] injected the arteries of a foetal cow with a black liquid, and found correctly that the uterine cotyledons were uninjected. The reverse experiment of injecting the uterine arteries was responsible for the erroneous belief that the placentae could be injected from the cotyledons. Monro, however, considers that in the latter case Slade's expression '"carried into the substance of the placentae" may signify no more than effused on their unequal pappy substance'.

The small tract by de Graaf, published in 1668,[3] is important not as an original contribution to the subject, but because it brings contemporary knowledge to a focus, and determines the fate of the injection method. He is the first to figure an injecting syringe of the modern pattern, and is credited with having injected mercury into the spermatic vessels. He says his attention had been directed to the subject of injections five years previously, owing to the great difficulty of tracing the blood-vessels by the methods of dissection then in use. By means of his syringe, however, it was possible to demonstrate all the arteries and veins of the body in a single day. Again, it was possible

[1] J. Pecquet, 'Une nouvelle découverte de la communication du canal thorachique avec la veine cave inférieure', *Journal des Sçavans*, Paris, 1672, 8vo.

[2] Matthaeus Slade (1628–89), *Dissertatio epistolica contra Gul. Harveum interpolata*, Amsterdam, 1667, 8vo.

[3] Reinier de Graaf (1641–73), *De Usu Siphonis in Anatomia*, Leyden and Rotterdam, 1668, 8vo.

to establish by experiment the circulation of the blood. A liquid injected into the carotid artery, after circulating through the brain, returned by the jugular vein, and if injected into the


FIG. 1. Engraved title of the first edition of the *De Usu Siphonis* (1668).

pulmonary artery it returned by the pulmonary vein to the left side of the heart. Other experiments of a similar nature are described. Thus de Graaf, as well as Malpighi and others, anticipates the publication of Harvey's pioneer experiment made in 1651. De Graaf describes the injection liquids used. They are

all watery preparations and have no permanency. A beautiful dark blue colour, he says, may be obtained by the action of sal ammoniac on copper. If this is found too difficult to prepare, use may be made of colours extracted from flowers, such as the violet, cornflower, and rose. All these colours may be changed by appropriate reagents, so that if only one extract is to hand other colours may be prepared from it. Further colours available are gamboge and indigo, and a green is obtainable by mixing the two. He recommends gamboge and indigo because they are easily procured, and, unlike sal ammoniac, do not affect the metal of the syringe.

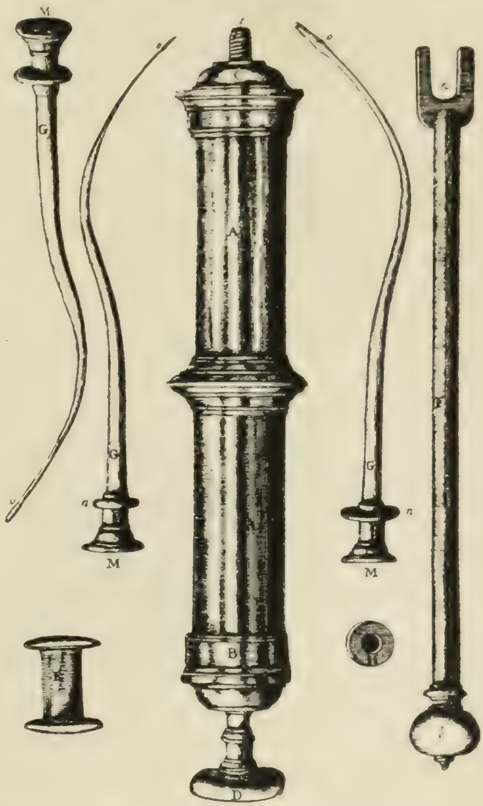



Fig. 2. De Graaf's injection syringe and accessories (1668)

A typical and important experiment is thus described by de Graaf. If the carotid artery is injected towards the head with the green medium one sees

'omnes partes, tam internas, quam externas, quae ab arteriis carotidibus sanguinem accipiunt, colore viridi tingi; quod iucundissimum spectaculum in cerebro exhibet, cuius superficies tam dextra quam sinistra, propter anastomosin, quam interse habent utriusque lateris arteriae ab iniecto colore viridi plurimas quasi arborum figuras repraesentare videntur, quae non sunt nisi arteriarum ramificationes per cerebrum excurrentes'.

Another example of de Graaf's ingenuity is to inject the coeliac artery with a green medium, and the mesenteric artery with a yellow. He was thus able to delimit in a very diagrammatic way the portions of the gut supplied respectively by these two vessels. Milk injected into the liver gives it a white colour, and similarly the gland becomes green if a green liquid is used, from which he concludes that the colour of the liver is dependent on

the contents of its vessels. The thoracic duct is injected and distended with milk, and the uterine vessels are inflated with air (1672). He also investigates the connexion between the vas deferens, seminal vesicles, and urethra by injections, and in the same way shows that the penis can be erected after death by injecting the internal iliac artery. His injection experiments were largely responsible for the debateable view, held until comparatively recently, that the Mammalian seminal vesicle acts as a receptacle for the spermatozoa.

De Graaf's syringe is not dissimilar to the modern instrument. It is made of copper or silver. The canula is long and bent, and is screwed directly to the syringe, being tightened with a key and the joint made good by a leather washer. There is no stopcock. The piston is packed with thread, and the key is drilled out to contain brass wires for cleaning the canula. Straus-Durckheim in 1843 states that the injection syringe had hardly been modified since the time of de Graaf, and that Swammerdam's methods were still in use.

In a paper dated December 17, 1668, King[1] describes the structure of the testis. He holds that, apart from the arteries and veins, the testis consists of massed tubules—' is a mere scheme or congeries of vessels '. He proves that they are hollow by injecting them with coloured spirit. These tubules, according to King, can be demonstrated in the rat by dissecting off the tunica albuginea and shaking them out in water, as first shown by de Graaf.

The Parisian comparative anatomists of the seventeenth century[2] made little use of injection methods, but the few experiments they describe deserve attention. They study the structure of the kidney of man and the lion by injecting milk into the renal vein, and are thus able to dispose of the statement of Vesalius that the factors of the renal vein arise in the centre of the kidney. If an injection be thrown into the pulmonary artery of a dog, it traverses the lung and emerges by the pulmonary vein much more easily and quickly if the lung be kept inflated by a pair of bellows. In the tortoise they investigate the relations of the epididymis

[1] Sir Edmund King (1629–1709), ' Observations concerning the organs of generation ', *Philosophical Transactions*, London, 1669, 4to, vol. iv, p. 1043.

[2] Claude Perrault (1613–88), *Mémoires pour servir à l'histoire naturelle des animaux*, Paris, 1671, 1676, fol.

and testis, and conclude that the former is simply a convoluted tube. 'Having made an injection of a coloured liquor into this ductus, a great many other little ductus's were made to rise, which did not appear before, and which went from the testicle to the epididymis: these ductus's being enclosed in the membrane which retained the circumvolutions of the epididymis, and which fastened it to the testicle.' This result, combined with that described by King in 1668, may be said to inaugurate our modern knowledge of the structure of the testis and its related ducts, but it was not until 1745 that Haller put the whole matter in a convincing light.

Swammerdam[1] is usually regarded as the inventor of the solidifying injection mass. We have already seen that this claim cannot strictly be maintained, but it is also undeniable that Swammerdam stereotyped the method, and was responsible for its general adoption by anatomists after his time. Priority of publication may belong to Boyle and Pecquet, but it is to Swammerdam, one of the greatest practical anatomists of all time, that the credit rightly belongs. According to the life by Boerhaave, who is followed by Straus-Durckheim, the first wax injection was made in van Horne's house in Leyden on January 22, 1667, but this date is obviously an error, and is in fact contradicted not only by another passage in the same work, but also by Swammerdam himself, who states that in 1666, 1669, and 1670 he demonstrated his method to van Horne, Slade, Thévenot, and Steno. His results were to some extent incorporated in J. van Horne's Prodromus of 1668, but the facts seem to be that whilst van Horne suggested the wax method, it was Swammerdam who first reduced it to practice. Swammerdam says: 'Factum est, ut D. van Horne proponerem, structuram venarum et arteriarum beneficio iniectae rubrae vel virentis cerae detegi posse.' J. Hudde (1640–1704), the eminent Dutch mathematician, also proposed to Swammerdam the use of coloured injection media. Boerhaave adds that the three plates of six figures were sent to the Royal Society on May 1, 1672, accompanied by the actual specimen itself. The body of the published work is dated March 5, 1670 [1671], and the appendix May 1, 1672. The preparation of the uterus was in the collection

[1] Jan Swammerdam (1637–80), *Miraculum naturae, sive uteri muliebris fabrica*, Leyden, 1672, 4to.

of the Royal Society in 1681, when it was catalogued by Grew in the following words: 'The womb of a woman, blown up and dried. Together with the *spermatick vessels* annexed; and the *arteries* in the bottom of the *uterus*, undulated like the claspers of a vine; all filled up with soft wax. Also the membranous and round *ligaments* of the womb, the *ureters*, *bladder*, *clitoris*, *nymphae*, *hymen*, *Fallopian tube*, and the *ovarys*, commonly called the

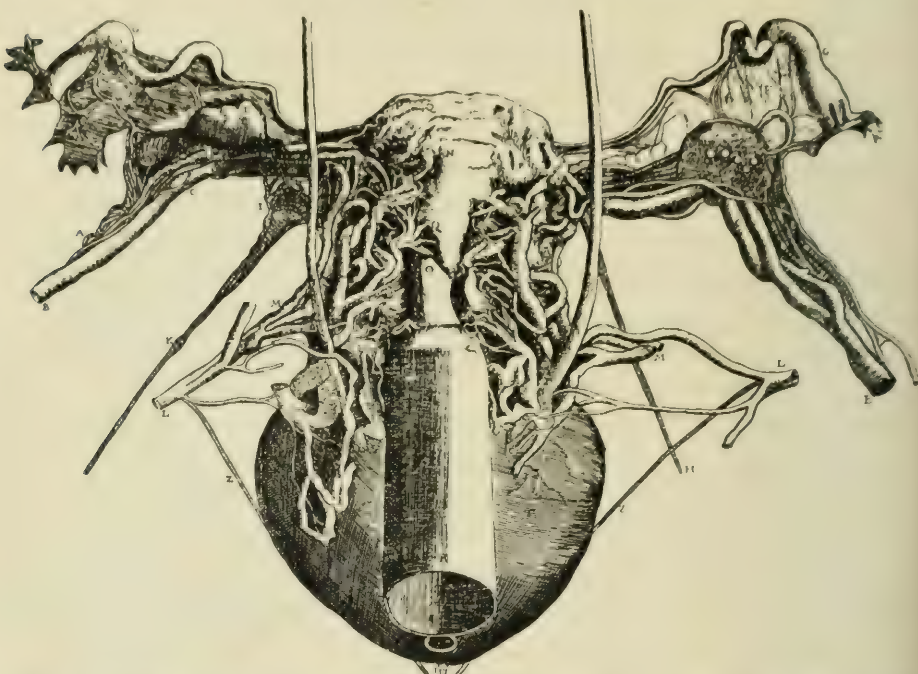


FIG. 3. Uterus injected with red wax by Jan Swammerdam (1671), who presented this specimen to the Royal Society.

*testicles*; all made most curiously visible, and given by Dr. *Swammerdam*. The descriptions and figures hereof may be seen in the same author's book, printed at *Leyden*, 1672, and presented to the *Royal Society*.' In 1781 the Royal Society's museum was handed over to the British Museum, and Swammerdam's injected specimens of the uterus, gall bladder, and spleen have since been lost sight of. Swammerdam gives a brief description of his method. He used pure white wax, to which, when liquid, the red, yellow, green, or other colour as required was added. The medium was injected as a hot liquid with a syringe, care being taken to avoid the admission of air into the vessels because this was found to

impede the injection. Before injecting, the blood was pressed out of the larger vessels in order that they might quickly and easily fill. When cool the wax solidified in the vessels, and the result was a permanent preparation which could not bleed if it should be necessary to dissect it. The discovery was communicated to his friend Ruysch, who hailed Swammerdam in emphatic language as the inventor of the wax method. The uterus described in 1672 was injected with soft red wax, blown up, and dried. Swammerdam also practised colouring the wax differently for arteries and veins, and had previously in 1667 attempted physiological injections of acid liquids into the veins of living animals. In his history of bees, first published in 1738,[1] there is an interesting description of how he injected the blood-vessels of a Lepidopterous larva. He says the blood-vessels of insect larvae are so very delicate and transparent that they cannot ordinarily be discerned,

FIG. 4. Swammerdam's method of injecting the small vessels of insects (first published 1738).

'though there are inventions of art, by the assistance of which we may come to the knowledge of them. In Silkworms I succeeded by the following method. . . . I provide myself with a little glass tube, . . . which I take care to have made like a vial in the middle, at one end to be drawn out to the utmost smallness, and at the other end made thicker and broader, in order that the air blowing into it, may be conveniently forced in at this end: this done, I fill the little pipe with some thin liquor coloured, not, however, of a very penetrating kind, let in through the thicker end, and then with the greatest caution perforating the skin, I thrust the thinner end into the heart. This may be done easily enough. By these means, and then gently blowing into it, the heart, and many of the vessels shooting out from it, may be filled.'

It is generally admitted that Frederik Ruysch, the Professor of Anatomy at Amsterdam, is the apostle of the injection method,[2] which was in fact for some time referred to as the 'Ruyschian

[1] J. Swammerdam, *Biblia Naturae*, Leyden, 1737–8, fol.

[2] Frederik Ruysch (1638–1731), *Opera Omnia Anatomico-medico-chirurgica*, Amsterdam, 1721–5, 4to.

art '. Even his opponent Bidloo admits that he was a ' subtle butcher ', who welcomed even a civil war which was to provide him with material for his studies. Ruysch was occupied with anatomy between 1665 and 1728, and from about 1675 onwards was recognized as the leading exponent of injection experiments. He was the intimate friend of de Graaf, Swammerdam, and Boerhaave. Ruysch's object was to produce finer and more complete and permanent injections than his predecessors. In the latter respect he was anticipated by Swammerdam, but he succeeded in being the first to obtain extensive injections of the blood capillaries. His preparations surprised ' even the most learned part of mankind ', and, to quote Boerhaave, he was the first to demonstrate that there were not two arteries distributed alike throughout the whole body, for ' in the liver they appear like small pencil brushes, in the testicles they are wound up like a ball of thread, in the kidneys they are inflected into angles and arches, in the intestines they ramify like the branches of trees, in the uvea they form circles and radii, in the brain they are waved in and out in a serpentine course, in the omentum they are disposed something like the meshes of a net, and in almost every other part of the body they assume a different and peculiar structure '. Already in 1664, as he tells us himself, Ruysch was making injected preparations of the spleen of the calf which were publicly demonstrated with applause by J. van Horne.

Galen and the ancients believed that there were parts of the body, to which they gave the name ' spermatic ', which had no vascular supply of any kind. This belief survived until the seventeenth century, when the fine wax injections of Ruysch demonstrated the occurrence of blood-vessels in bones, ligaments, tendons, and membranes—in fact in all living tissues almost without exception. Ruysch therefore may claim the credit of establishing the ubiquity of the vascular system.

Ruysch followed Swammerdam in injecting a substance which cooled in the vessels, and thus ensured the permanence of the preparation. His first attempts exceeded the achievements of Swammerdam, and vessels of extreme tenuity became visible to the naked eye. He was hence able to discover the thin fibrous periosteum of the auditory ossicles (1697), which had up to that time been regarded as naked, the vasa vasorum, the capillaries of the bronchial tubes, and the vessels of the choroid and other

nembranes. It is natural that he should exalt the significance of ıis own discoveries, and that he should regard this most subtle ınd intrusive vascular system as having an existence *per se*. In 1723, almost at the end of his life, his enthusiasm for injections s expressed in the following passage: 'Vah quantum est, quod ıescivimus ante repletionis artem! quantum didicimus per illam! ) mirus naturae in ultima vascula olim invisibilia appulsus, quo ;am abscondita manifestantur! O grata repletio, numquam con- ;emnenda deinceps, numquam laudanda satis!' He claims, for example, that by establishing the high vascularity of the cerebral cortex, he has disproved its glandular nature as asserted by Malpighi, and his substitution of the vascular for the glandular theory of the cortex tends to obscure the important advance which was actually made. He is unusually and unfortunately emphatic on this point. Of all the discoveries which he has been making for forty years, he says in 1705, the most important is the proof that the cerebral cortex is not a gland but only a mass of blood-vessels. His injections were so refined that the parenchymatous elements were overlaid and obscured by the multitude of vessels which sprang into view. This, and doubtless certain extravasation effects, induced him in 1696 definitely to formulate the doctrine that the tissues were only vascular networks variously arranged ('totum corpus ex vasculis'). Already in 1695 H. Ridley had stated this view, and as late as the eighteenth century William Hunter was still teaching that the glands consisted of a concourse of vascular elements and excretory ducts without any definite or apparent parenchymatous basis. Ruysch had injected the arteries and veins of certain glands, macerated them in water, and then unravelled them without finding anything but a tangle of injected vessels. Ruysch, however, did not hold that a gland was formed wholly of blood-vessels, but admitted that between the extremities of the vessels there existed a neutral pulpy substance. He believed, further, that the blood was poured directly into the factors of the excretory ducts of the glands, since his


FIG. 5. Periosteum of the auditory ossicles injected by Ruysch (1697).

wax injection thrown into an artery emerged by the excretory duct without entering any intermediate non-vascular tissue or spaces on the way. 'The ceraceous injection of Ruysch being artfully impelled makes its way [via the uriniferous tubules] even into the pelvis of the kidney from the arteries' (Boerhaave). 'This was the principal ground for the famous, but now exploded theory of the existence of exhalant arteries with open mouths, which in the secretory glands opened directly into the excretory ducts' (Bowman). On this view there was no need for a parenchyma, but Ruysch was unable to deny the existence of a non-vascular tissue in the lingual glands, although he maintained that the lobules of the liver consisted of bunches of vessels without any parenchyma. His views on the ultimate structure of animal tissues were strongly supported by Cowper and Nuck.

As a result of Ruysch's experiments there arose a 'despotism of injections', which attributed the functions of the tissues to the specific disposition and peculiarities of their vascular supply. This disposition was believed to exist in infinite variations, and the diverse activities of the tissues were explained as the necessary consequence of such variations. Malpighi, who had correctly stated the relations between glandular tissue and its blood supply, was thus strongly opposed and beaten down by Ruysch and his followers, who denied the very existence of glandular tissue, and saw nothing in a gland but a subtle complex of blood capillaries. Two exceptions to this vascular autocracy were admitted—the ovary and the testis, the fabric of which Ruysch was never able to prepare by injection, and he concluded therefore that red blood did not penetrate into the essential parts of these glands. Nuck and Cowper (1697) also failed to inject the testis.

Another consequence of Ruysch's injections was an improved method of preserving bodies from putrefaction, for which his name became famous in all the schools of Europe. In 1666, by order of the Dutch Government, Ruysch, who was then in his twenty-eighth year, undertook to preserve by vascular injection the body of Vice-Admiral Sir William Berkeley, whose ship had been captured, and the admiral himself killed, when leading the van in the gallant but hopeless contest of June 1. The experiment, though made more difficult by the putrefaction of the body and the admiral's wounds, was wholly successful, and the Dutch chivalrously returned the body to England in condition as fresh,

we are told, as that of an infant. Ruysch was encouraged to attempt other preparations of a similar nature for his own Museum, which has been described as a ‘ perfect necropolis, all the inhabitants of which were asleep and ready to speak as soon as they were awakened ’. Ruysch himself says: ‘ Sunt mihi parvula cadavera, a viginti annis balsamo munita, quae tam nitide sunt conservata, ut potius dormire videantur, quam exanimata esse corpuscula.’ Entire bodies of infants and adults were mummified by vascular injection of possibly some preparation of arsenic, with what success the following paraphrase of an eloquent passage in Eloy provides ample testimony:[1]

> All the bodies which he injected preserved the tone, the lustre, and the freshness of youth. One would have taken them for living persons in profound repose—their limbs in the natural paralysis of sleep. It might almost be said that Ruysch had discovered the secret of resuscitating the dead. His mummies were a revelation of life, compared with which those of the Egyptians presented but the vision of death. Man seemed to continue to live in the one, and to continue to die in the other.

A quaint manifestation of Ruysch's interest in injections is to be found in his Museum. The skeletons are thrown into dolorous attitudes and provided with anatomical pocket handkerchiefs of injected omentum, their wrists are adorned with organic and injected frills, and even the bladder used to seal the mouths of the jars has been carefully injected. Of the methods by which his wonderful preparations were produced we have but scanty knowledge. Even his friend Boerhaave, who was occasionally present when Ruysch was injecting, is silent as to his methods. When Peter the Great, on his second visit to Amsterdam, acquired Ruysch's collections in 1717, having first seen and coveted them in 1698, it is said that he stipulated that the preparations should be accompanied by a description of the methods of the preparateur. This was accordingly drawn up by Ruysch, and ultimately found its way into the library of the University of St. Petersburg, founded in 1819. In 1742 an account of this manuscript, based on the copy in Ruysch's handwriting, was published by Joannes Christophorus Rieger. Rieger had been in the employ of Peter the Great, after whose death in 1725 he retired to Holland, where

[1] Obviously inspired, however, by a passage in Fontenelle's *Éloge de Ruysch*, published in 1731, which itself owes something to the rhetoric of Ruysch himself.

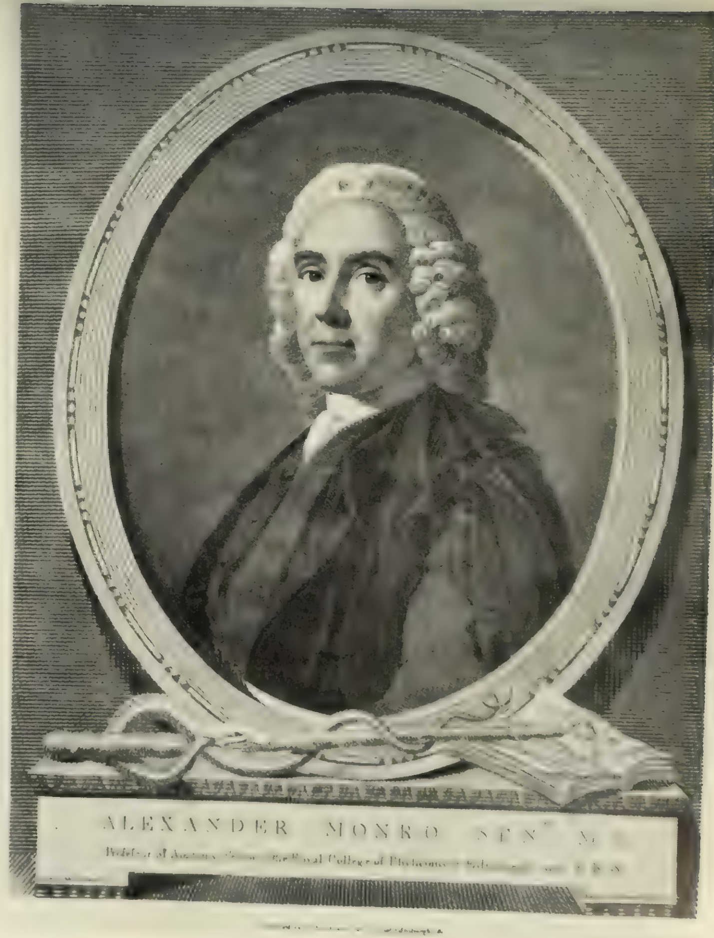
he lived over a bookseller's shop, and compiled the work which includes the description of Ruysch's methods. He states that he saw the manuscript by permission of the President of the Imperial Academy of St. Petersburg. Now, the Academy was only founded in 1725, so that he must have examined the manuscript before his retirement to Holland, and after the death of Peter the Great. It seems probable that Ruysch's collections and manuscript were for a time handed over to the care of the Academy, since a description of the preparations was issued by that body in 1741.[1]

From Rieger we learn the following details of Ruysch's procedure. Macerate the body to be injected in cold water for a day or two. Slit up the aorta and vena cava and press out the blood, and immerse the body in hot water from four to six hours. The injection used is in the winter suet or tallow, and in the summer a little white wax is added. Another mass may be prepared by mixing wax, turpentine, and resin. The colour employed is vermilion, or spirits of wine and vermilion, and the larger vessels are afterwards filled with wax to prevent the escape of the spirit. Two tubes are fixed in the aorta, one being directed forwards and the other backwards, and the vena cava is ligatured. The vessels are filled by means of two heated syringes, and the subject is to be moved about continually in cold water after the operation to prevent the gravitation of the heavy vermilion whilst the mass is setting. Having been injected the specimen is preserved in diluted alcohol, which Ruysch distilled himself from barley. Black pepper was added to assist its penetrative power. The strength of the alcohol was only about 67 per cent.—too weak for the purposes of permanent conservation, although no deterioration was observable during Ruysch's own lifetime. Hyrtl suggests that the secret was handed over to Rieger when it was seen that the preparations were becoming worthless, and we have the statement of Jesse Foot, published in 1794, that ' I saw the preparations, belonging to Ruysch, which are deposited in the Museum at Petersburg, going apace into decay ', and before this, in 1748 Lieberkühn, who had examined examples of Ruysch's injection mass, considered it too fluid to last, whilst the preparations themselves did not stand microscopic examination. In order to display the

[1] Ruysch's second collection was catalogued by himself in 1724 and 1728, and by Abraham Vater, who had been one of his pupils, in 1736–40.

FRIEDRICH RUYSCH

From the first edition of his collected works (1721)




ALEXANDER MONRO *PRIMUS*

From his collected works (1781)

injected microscopic vessels Ruysch cleared the tissues with oil of lavender or turpentine.

It is obvious in the Rieger document that Ruysch conceals more than he discloses. In the subsequent literature there are many conjectures by various anatomists as to the composition of Ruysch's media, but none of them carry the authority or conviction of the definite if meagre statements of Rieger. Besides injection, Ruysch was very successful in the use of the inflation method. The lymphatic vessels were inflated by means of glass tubes, dried, and dissected so as to expose the valves, of which Ruysch, though not the discoverer, was the earliest to publish a careful study in his first paper issued in 1665.

From Ruysch to Lieberkühn, who established the importance of microscopic injections, there is an extensive literature—not, however, of sufficient interest to be dealt with other than briefly. Blankaart [1] seems to have been the first to demonstrate *by injections* that the connexion between arteries and veins was not by a spongy parenchyma but by capillaries, a conclusion already reached by Malpighi, and confirmed a few years later by Lange [2] and Leeuwenhoek (1689). Caspar Bartholin,[3] the son of Thomas and the grandson of old Caspar, published two works dealing with injections when he was still very young, in the earlier of which he was accused by his contemporaries of subtle plagiarism. His advance on de Graaf is that he was the first to recommend systematically flushing out the vessels with water before throwing in the coloured injection, thus anticipating a modern refinement. He proceeds as follows: the part is steeped in tepid water in order to soften the clotted blood, which is then removed from the vessels by an injection of warm water. This preliminary operation is facilitated by ligaturing the return vein and suddenly releasing the ligature, when the sharp rush of the hot water carries the blood with it. He claims for this method a finer and more general injection, but this was questioned by later writers, who asserted that the water left the vessels, and produced misleading infiltra-

[1] Steven Blankaart (1650–1702), *Tractatus novus de circulatione sanguinis*, Amsterdam, 1676, 12mo.

[2] Christian Johannes Lange (1655–1701), *Disputatio de circulatione sanguinis*, Leipzig, 1680, 4to.

[3] Caspar Bartholin (1655–1738), *De diaphragmatis structura nova*, Paris, 1676, 8vo. *Administrationum anatomicarum specimen*, Frankfurt, 1679, 8vo.

tions in the surrounding tissues. Bartholin's apparatus was either a modified syringe or a specially constructed machine devised to facilitate his irrigation experiments. The principle of both, however, is the same, viz. that an unlimited supply of the injection fluid can be forced into the vessels without withdrawing the nozzle from the artery. This is effected by a T-piece and valves, but he admits that, owing to temperature difficulties, his contrivance is not suitable for wax injection, for which the ordinary syringe must still be used. Bartholin was the first to devise an injection apparatus with a continuous feed, but it found little favour until Straus-Durckheim in 1843, and Robin in 1849, developed the idea and recommended a more complex form of it. It is to be noted that de Graaf and Bartholin are the first to figure injection syringes. Bartholin injected air, water, and various coloured liquids, but does not favour wax. He produces a green by mixing gamboge and indigo, as already described by de Graaf.

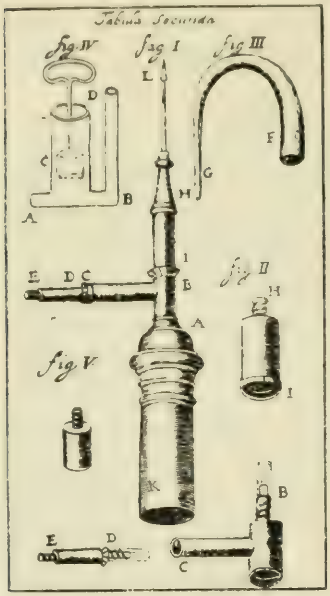


FIG. 6. Injection appliances of Caspar Bartholin (1679). The syringe can be recharged without disconnecting the apparatus.

Duncan is perhaps the earliest writer definitely to advocate the use of mercury,[1] and to contrast the arteries and the veins by injecting them with different colours. He employs usually melted wax thinned with turpentine and oil, and also a black liquid. His wax injection is 'according to the method of M. Swammerdam', except that he recommends that as the wax hardens quickly it is better to inject the animal when it is still alive! He refers to Swammerdam also as experimenting with mercury for injections. Duncan injected one colour first by the jugular vein, and then another colour by the vertebral artery. 'Thus the arteries and veins are easily distinguished by their different colour', and the communications between the arteries and veins are demonstrated. The experience which the great comparative anatomist Duverney derived from injections was strangely

[1] Daniel Duncan (1649–1735), *Explication nouvelle et mécanique des actions animales*, Paris, 1678, 12mo.

unfortunate.[1] In two papers written in 1679 and 1683, but not published until 1733, he anticipates the speculation of Ruysch by stating that the so-called solid tissues of the body are nothing but a miraculous concourse of different vessels, and that when all the liquids have been expressed from a tissue nothing is left but canals and vesicles. In the Stork his injections failed to reveal the lacteal vessels or the thoracic duct, the occurrence of which in Birds he therefore doubts. An injection thrown into the mesenteric vein passes into the cavity of the intestine, and conversely if a portion of the gut be filled with milk, ligatured at both ends, and then compressed, the milk is forced into the mesenteric vein. He holds that the chyle in the Bird passes via the mesenteric veins to the liver.

Dismissing Charleton,[2] who refers to ' various manual operations besides meer dissection ', such as ' inflations, injections of divers liquors by syringes ', Simon Lescot (*c.* 1615–90), the surgeon who is supposed to have introduced Swammerdam's wax method into France *c.* 1680, and de Heyde, who practised physiological injections on living dogs,[3] we come to Bidloo,[4] who inaugurated an injection experiment which, applied as it afterwards was to the blood system, became very popular in the eighteenth century. He filled the lungs with ' fused bismuth ', afterwards removing the soft parts by corrosion, thus producing a permanent cast in metal of the cavities of the lung. By ' fused bismuth ' must be understood not the pure metallic substance, which melts at too high a temperature for the purpose, but a complex mixture containing bismuth and mercury, some preparations of which had almost the consistency of wax.

The English comparative anatomist Samuel Collins [5] has never achieved the reputation which his work undoubtedly deserves. It is contemptuously regarded by Hutchinson as ' of less value than the head that is placed before it '—referring to the beautiful

[1] Joseph Guichard Duverney (1648–1730), *Histoire de l'Académie Royale des Sciences*, Paris, 1733, 4to, T. 1, pp. 278 and 363.

[2] Walter Charleton (1619–1707), *Enquiries into Human Nature*, London, 1680, 4to.

[3] Anton van der Heyde, *Centuria Observationum Medicarum*, Amsterdam, 1683, 8vo.

[4] Govard Bidloo (1649–1713), *Anatomia humani corporis*, Amsterdam, 1685, fol.

[5] Samuel Collins (1618–1710), *A System of Anatomy*, London, 1685, 2 vols., fol.

engraving of Collins by Faithorne which constitutes the frontis-piece. Collins, however, made little use of injections, and his few experiments served but to mislead him. He employed white wax and vermilion, and as a result adopted views on the relations of

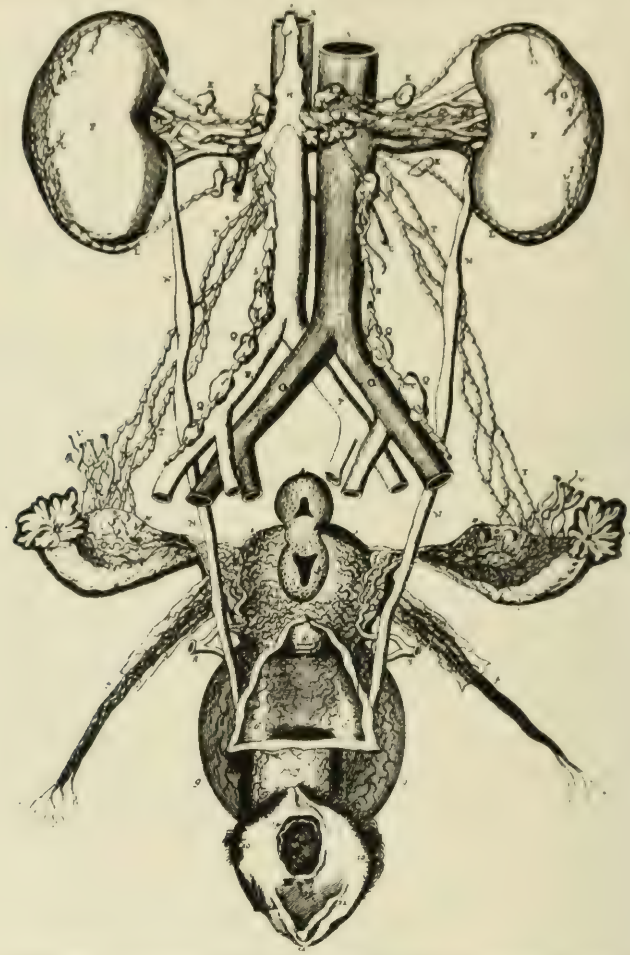


FIG. 7. The lymphatics of the urogenital organs injected with mercury by Anthony Nuck (1691).

the cephalic and genital arteries and veins contrary to the knowledge even of his own time. His contemporary Nuck,[1] on the other hand, wielded the syringe with admirable results. He injected red coloured wax and coloured fluids. The wax was mixed with oil to make it sufficiently fluid to reach the finest branches, but his chief medium was mercury, which he was the

[1] Anthony Nuck (1650–92), *De ductu salivali novo*, Leyden, 1685, 16mo. *Adenographia curiosa*, Leyden, 1691, 8vo.

irst to put to extensive use. He also experimented with an ımalgam of mercury and lead or tin, and a preparation called inctura mercurialis, the composition of which is not given. Nuck's general description of the lymphatics is one of the most complete before Mascagni. He investigated the structure of the secretory glands by injecting their ducts, lymphatics, and arteries and veins, and also by inflation. He was the first to demonstrate, by means of mercury injection, that the lymph stream passes through and beyond the lymphatic glands and is not interrupted by them. He was also the first thoroughly to explore the constitution of the secretory and lymphatic glands and of the lymphatic system by mercury injections. But his experiments led him into numerous errors. He found that mercury passed from the lacteals and lymphatics into the arteries, and concluded from this that the lymphatics arose from the arteries. In the same way he deduces that the ramifications of the arteries are connected up with the factors of the salivary ducts. He inflated the arteries of the spleen and kidney and saw air pass into the lymphatics, and hence believed that the lymphatics were veins. It was not until 1757 that Monro secundus finally demonstrated that Nuck's errors were due to extravasation effects. A similar misconception was based on his injection with mercury of the glandular lobules of the mammary gland, when the mercury found its way into the blood-vessels—especially the arteries.

The botanist Camerarius [1] was one of the pioneers of mercury injection. He injected the testis from the vas deferens, and other organs, with milk, wax of diverse colours, and mercury, and also inflated them with air. Compare the preceding and later work on the injection of the testis of Perrault, King, and Haller. The inevitable mercury extravasation induced the belief that there was a connexion between the seminal tubules and the lymphatics. In 1686 A. van der Heyde injected the gastro-vascular canals of Aurelia with a black liquid. Contrary to expectation Leeuwenhoek [2] does not appear to have practised injections himself, but he mentions injections of hot wax, and gives the following interesting description of a mercury injection: ' A certain doctor of Physic,

[1] Rudolphus Jacobus Camerarius (1665–1721), ' De nova vasorum seminiferorum et lymphaticorum in testibus communicatione ', *Ephem. Acad. Nat. Cur.*, Ann. 1686, 1688, Nürnberg, 1687, 1689. Dec. 2, Ann. 7, p. 432, 4to.

[2] Anthony van Leeuwenhoek (1632–1723), *Vervolg der Brieven geschreven aan de Koninglijke Societeit tot London*, Delff, 1689, p. 336, 4to.

to whom, among other persons, I had shown the circulation of the blood, told me that this circulation had also been exhibited to him by a chirurgical gentleman; and on my desiring to know how it was shown to him, he said by injecting quicksilver into an artery, which circulated back again through a vein; but when I asked him how they were assured that one of the vessels in which the experiment was made was an artery, and the other a vein, he answered that they were not certain as to that point. I also asked him what was the size of the vein in which the quicksilver, so injected, was circulated; to which he answered, that it was above a thousand times larger than those vessels in which he had seen the circulation of the blood at my house.' Leeuwenhoek was not convinced by this experiment, holding that it was possible the injection might return by an artery instead of a vein, and thus not necessarily demonstrate the circulation. On the other hand, Ray [1] believes that the structure of the body may easily be detected by blowing air into the vessels and drying the preparation, or by injecting melted wax or quicksilver with syringes. He accepts the doctrine of the tubular (i. e. non-parenchymatous) structure of the glands, and considers it wonderful that 'all the glands of the body should be congeries of various sorts of vessels cur'd, circumgyrated, and complicated together, whereby they give the blood time to stop and separate through the pores of the capillary vessels into the secretory ones, which afterwards all exonerate themselves into one common ductus'. In his work on the brain Ridley [2] found injection experiments very helpful. He says: 'Other bodies have been introduc'd by injection, as *tinged wax* and *mercury*, the first of which by its consistence chiefly, the other by its permanent nature and colour, contribute mightily towards bringing to view the most minute ramifications of vessels, and secretest recesses of Nature.' He prefers mercury to wax, the latter being too coarse for the finest vessels. 'By an injection with mercury I find scarce any nerves but what hath some such small ramifications of blood-vessels in them.' His 'chief hopes' were based on mercury, but he is almost the first to recognize that extravasations might make mercury injections 'altogether useless'.

[1] John Ray (1628–1705), *The Wisdom of God manifested in the works of the Creation*, London, 1691, 8vo.

[2] Humphrey Ridley (1653–1708), *The Anatomy of the Brain*, London, 1695, 8vo.

The Ruyschian art found an enthusiastic supporter in the arch-plagiarist Cowper.[1] Besides inflating the vessels with air and drying the preparation, he used as injection masses plaster of Paris, wax, and mercury. With the latter he injected the biliary ducts, kidney, lymphatic system, and carotid artery. He demonstrates by mercury injections that the maternal blood-vessels penetrate into the placenta,[2] but although he found, as related by Drake, that mercury injected into the uterine artery of a cow passed via the cotyledons into the foetus, he hesitates definitely to commit himself to the continuity of maternal and foetal blood. There was a strong tendency at the time to believe so natural an assumption, as witness the following passage from Drake (1707): 'On the other hand, if the arteries of the uterus were continued to the veins of the same part, and those of the foetus in like manner, without communicating with each other, their confluence in the placenta seems to be altogether impertinent and of no use, and the umbilical arteries and vein fram'd for no other service or purpose, than to give the blood room for an idle sally.' Cowper injected mercury into the lactiferous tubes of the mammary gland, and he confirms Nuck in finding it emerge by the blood-vessels. His injection experiments lead him into many other errors, as, for example, when he describes a mass thrown into the renal artery escaping by the ureter. Mercury is used, and also variously coloured wax, to inject other than vascular cavities, e. g. the Fallopian tubes. The arteries of a foetus are demonstrated by a wax injection. He uses wax of four different colours to inject the liver—hepatic artery, red; portal vein, 'dark' colour; post-caval, 'distinguishable' colour; and bile duct, yellow. The liver lobules were next roughly separated, slightly macerated, and the parenchyma removed with a stiff brush of hog's bristles. Wax is injected into the salivary and pancreatic ducts to ascertain how these ducts are formed within the gland. Cowper also filled the pulmonary passages with 'block tin', and macerated off the soft parts. These preparations of liver and lung are among the earliest corrosion experiments of which we have any record, but the idea is obviously borrowed from Bidloo, who in his turn owes something

[1] William Cowper (1666–1709). *The Anatomy of Humane Bodies.* Oxford, 1698, fol. Some copies are dated 1697, but the work was not published until May, 1698.

[2] Monro concludes that on this point Cowper is speaking *à priori*, and not from what he actually saw.

to the excarnation preparations of Spigelius and Glisson described in 1627 and 1654. Block tin in the seventeenth century was a more or less pure tin as now, and hence on account of its high melting-point could not have been employed as Cowper states. He must therefore have made use of an alloy.

A short but important paper by Homberg was written in 1699, but not published until 1702.[1] Homberg experimented with wax, mercury, and turpentine—not the modern oil of turpentine, but a resin of the consistency of honey. He is also the first, if not to introduce, at least to demonstrate the practicability of injecting metals of low fusibility. His metal injection consisted of equal parts of lead, tin, and bismuth, which remains liquid at a temperature below the scorching-point of paper. Subsequently Newton and d'Arcet found that by altering the proportions of these three ingredients, and principally by increasing the percentage of bismuth, the melting-point was reduced, and the addition of mercury carried the reduction still further. Homberg finds the consistency and melting-point of wax and turpentine unsuitable for injection, whilst mercury escapes from the smallest cut. He believes the principal difficulty to be air in the vessels, and he professes to have overcome this difficulty by means of a complicated piece of apparatus which inflated and dried the vessels, thus facilitating the escape of the air. This apparatus, however, was afterwards discarded as an unnecessary refinement, and he next tried the interesting experiment of exhausting the air in the vessels by means of an air pump before throwing in the injection—a method said to have been one of the secrets of Ruysch. Then he runs in the hot metal as above, and after it has cooled, he removes the soft parts by maceration or otherwise, so that he has finally a permanent metal cast of the cavities of the vessels. He is alive to the danger of attempting such an injection on material which has been in water, and he recommends that such material should be first dried for a day in the air pump. Homberg is undoubtedly the pioneer of the so-called corroded preparation—a method which lost interest in the nineteenth century but was never absolutely abandoned, whilst recently the popular use of the low fusible alloys has resulted in its re-introduction.

In the first half of the eighteenth century the sciences of

[1] Guillaume Homberg (1652–1715), 'Essais sur les injections anatomiques,' *Hist. de l'Académie Royale des Sciences*, Paris, 1702, Ann. 1699, 8vo.

Anatomy and Physiology were making great progress principally by means of injection methods. All the literature of the period refers to the still fascinating doctrine of the circulation, and the injection syringe was the most popular and trusted instrument of the time. Hovius [1] worked out the circulation in the iris, ciliary process, and lachrymal gland by injections of wax and mercury. He describes vessels in the cornea, although none are present in a state of health, and professes also to have discovered vessels in the lens and humours of the eye, and to have established a definite circulation in those parts. Méry's injections of air and water [2] represent a very definite advance towards an understanding of the mechanism of respiration, but the unsoundness of his methods, as pointed out by G. B. Bülffinger in 1732, was against a more important result.

Vieussens' injection experiments [3] can only be generally condemned, and it is doubtful whether he obtained a single valid result. In his earlier work on the brain he used spirits of wine or brandy tinged with a saffron colour, and also black and green liquors and ink, but he gives no account of his material or procedure. Only local injections are described. Thus coloured spirits of wine pumped into the carotid artery reaches the longitudinal sinus directly from the arteries of the dura mater—an error afterwards corrected by Ruysch and Rau. His later publications are more detailed as regards injections, and he is now using mercury. His injections of the latter substance, obviously conducted at too great a pressure, result in deplorable errors. Mercury thrown into the trachea passed into the blood-vessels of the lungs, if injected into the mesenteric artery it escaped into the cavity of the intestine, from the cystic artery it found its way into the gall-bladder, and from the uterine arteries it penetrated into the cavity of the vagina but not that of the uterus. As the result of his injections he deduces that there is a connexion between the arteries and the lymphatic system. The lymphatic duct is regarded as a lymphatic-

[1] Jacobus Hovius, *De circulari humorum ocularium motu*, Inaug. diss. Utrecht, 1702, 4to. Editio nova, Leyden, 1716, 8vo.

[2] Jean de Méry (1645–1722), *Histoire de l'Académie Royale des Sciences*, Paris, 1703, Ann. 1700. Ibid. 1708, Ann. 1707, 8vo.

[3] Raymond Vieussens (1641–1716), *Neurographia Universalis*, Lyons, 1684, fol.; *Novum vasorum corporis humani systema*, Amsterdam, 1705, 8vo.; *Dissertatio anatomica de structura uteri et placentae muliebris*, Cologne, 1712, 4to; *Expériences et Réflexions sur la structure et l'usage des viscères*, Paris, 1755, 8vo.

arterial-nervous apparatus, and he invents a new class of short-circuit vessels to explain the rapidity with which liquids taken into the stomach are removed by the kidneys, to account for which, he claimed, the Harveian circulation was too slow. Again, he injected the left coronary artery with coloured brandy, and describes the fluid as passing 'without violence' not only into the entire substance of the left auricle, but also into its cavity. Hence we have another of his theories that the coronary arteries and veins are not connected up by capillaries but by the cavities of the heart, with which both arteries and veins communicate by conspicuous apertures. An experiment of Vieussens which obtained wide currency and belief was the injection of the carotid artery of a living bitch with four pounds of mercury. He holds that the mercury 'without breaking any vessels, or the effusion of one drop of blood, passed through the placenta surrounding each whelp, and was pushed into the umbilical vessels themselves'. This experiment was twice repeated by Monro with quite different (but correct) results. The latter states that the mercury travelled into the very minute branches of the vessels of the maternal placenta, but none whatever reached the umbilical vessels or the foetus.

Noues,[1] who used rectified spirits with cinnabar ground in, and speaks of injections of coloured wax, has little of importance, but Schacher [2] carries Homberg's experiment a stage further, and is the first to *draw* the injection through the vessels with a vacuum pump. His mass, besides starch, is pig's fat, mutton suet, and spermaceti, each in combination with wax, to facilitate the filling of the smallest vessels, and the colours are vermilion, red oxide of lead, verdigris, Florence lake, the blue extract of corn-flower, and carmine. At this time Salzmann [3] describes injecting the thoracic duct from the lymph vessels in the neighbourhood of the renal vein, and Cheselden [4] injects the blood-vessels with wax and the lymphatics with mercury, but does not discuss his procedure. He

[1] Guillaume des Noues, *Lettres de Mr. de Noues à Mr. Guillielmini*, Rome, 1706, 8vo; *Avertissement pour les anatomies toutes nouvelles de cire colorée*, Paris, 1717, 12mo.

[2] Polycarpus Gottlieb Schacher (1674–1737), *De anatomica praecipuarum partium administratione*, Leipzig, 1710, 4to.

[3] Johannes Salzmann (1672–1738), *Dissertatio encheiresis inveniendi ductum thoracicum*, Strassburg, 1711, 4to.

[4] William Cheselden (1688–1752), *The Anatomy of the Humane Body*, London, 1713, 8vo.

points out that the lymphatics of fish, first observed by T. Bartholin, may be seen in the mesentery without injection. Injection as a method of preservation was practised by Ravius,[1] many of whose preparations in the Anatomy Hall at Leyden were catalogued by his successor, B. S. Albinus, in 1725.

An early, if not the earliest, attempt at histological injection is described by Muys.[2] He first of all removed the blood from the vessels by an injection of warm water, and then threw in a coloured liquid. His object was to ascertain the vascular supply of muscle fibres, and he wrongly concluded that the muscle fibrillae were tubular, and that the capillary artery discharged 'a part of its liquor' into the cavities of the fibrillae. The latter, however, were too small to admit the coloured blood corpuscles, which therefore underwent fragmentation before entering the fibre. A figure supports this flight of the imagination. The short paper by Rouhault, written in 1718,[3] is important and exceptional, since no attempt is made to conceal laboratory methods. He says the mass usually employed is a mixture of hog's lard, white wax, mutton suet, and turpentine, or spirit of turpentine charged with a little wax. His colours are vermilion for arteries and verdigris or indigo for veins, and he claims to be the first to employ different colours for arteries and veins. In this, as we have already seen, he is mistaken. Before throwing in the mass, the blood is cleared out of the vessels, and the body warmed, by an injection of tepid water, after which the body is wrapped in warm linen. The above injection does not pass through the capillaries, and he only uses it to demonstrate the coarser circulation, admitting that it gives preparations inferior to Ruysch's. We have pointed out that Boyle was the first to suggest isinglass as an injection mass, but Rouhault may claim the credit of demonstrating its importance. He was not aware of Boyle's work, and the use of gelatine was suggested to him by Méry. He dissolved Ghent glue or isinglass in water, and in 1716 obtained a perfect injection of the finest vessels, the mass, for example, passing right through the placenta

[1] Johannes Jacob Rau (1668–1719), *De methodo anatomen docendi et discendi*, Leyden, 1713, 4to.

[2] Wyer Guillaume Muys (1682–1744), *Journal Littéraire*, La Haye, Jan., Feb., 1714, 8vo.

[3] Pierre Simon Rouhault (ob. 1740). 'Sur les injections anatomiques', *Histoire de l'Académie Royale des Sciences*, Paris, 1720, 8vo, Ann. 1718.

and emerging by the veins. He exhibited at the Academy the vessels of the placenta injected with different colours. Comparing his preparations with some believed to have been injected by Ruysch's method, he considered his own as good as Ruysch's, which latter he adds, had *not* been injected with wax. Rouhault also used spirits of wine coloured with orchanette or carmine, which penetrates equally well into the finest vessels, but, like mercury, the preparation cannot be dissected afterwards without bleeding, and hence this method is only suitable for whole preparations.

Boerhaave,[1] though less a practical anatomist than a commentator, brings to bear on the subject so intimate a knowledge of his contemporaries and their methods that his observations invite attention. He does not believe that the minutest vessels are demonstrated even by 'the most subtile coloured liquor'. For example, while the membranes of the testis are richly provided with blood, the 'pulp of the testicle is supplied with pellucid juices by arteriolae much smaller than the sanguiferous, and into which the Ruyschian injection will not enter'. It was commonly noted at the time, in the case of vermilion masses, that the waxy or other vehicle penetrated into vessels which would not admit the vermilion, and Boerhaave believed that the pressure exerted by the injector forced the injection into vessels which were not normally traversed by red blood, 'since no part of the cortex ever appears red without injection'. He thus supports Malpighi as against Ruysch in the controversy on the structure of the glands, holding that Ruysch's injections unnaturally dilated the blood capillaries and thus obscured or even destroyed the glandular tissue, nor will he assent to Ruysch's view that injection mass passes from the arteries into the excretory ducts of a gland. Boerhaave makes one statement the origin of which the writer has not been able to trace. He says that some English anatomists injected the carotid arteries with urine coloured with ink (a medium employed by Malpighi), and found that the nerves of the brain were tinged with the colour. When one of the nerves was sectioned it appeared full of black specks, which were supposed to represent the cavities of the nerve tubes. It will be remembered that at this time the theory of Descartes that nerve fibres were tubes transmitting fluid contents was generally accepted.

[1] Herman Boerhaave (1668–1738), *A method of studying Physic*, London, 1719, 8vo.

In 1720 Valentin [1] gives formulae for injecting the alveoli of the lungs and the blood-vessels with low fusible metals, the flesh being afterwards boiled away, leaving a cast of the injected cavities. He uses also a wax medium made of white wax mixed with mutton suet and coloured with verdigris or vermilion, which passes through capillaries and sets when cold. Albinus,[2] who claimed that the art of injecting coloured liquids into the vessels was a Belgian invention, employed a red liquid which stiffened when the vessels were distended with it. His injections were finer than Ruysch's, and he filled the minutest vessels in the skin, brain, and capsule of the lens. The latter, however, must have been in the foetus, as there are no vessels in the capsule of the adult. William Hunter, who examined his injections in 1748, was deeply impressed by them. In 1756 Albinus is combating vigorously Ruysch's doctrine that the human body is composed entirely of vessels.

Between 1721 and 1732, when Monro primus published his first paper on injections, several writers deal with the subject, but little is added to what was already known. Helvetius [3] supports Ruysch in holding that the whole body is almost nothing more than a prodigious assemblage of lymphatic and blood vessels, but considers that injections are deceptive in attracting too much attention to the blood-vessels. The latter belief finds an apt illustration in the work of Stukeley on the spleen,[4] whose injections of wax in two colours induced him to adopt the view that the spleen consists almost entirely of the ramifications of the splenic artery. He admits the presence of a few muscular fibres, but even the veins, according to this author, are only slightly concerned with the internal economy of the gland. According to William Hunter, who as a contemporary might be expected to know, Nathanael St. André (1680–1776) was one of the early English injectors, and Haller asserts that St. André claimed to have injected vessels in the epidermis with quicksilver. This

[1] Michael Bernard Valentin (1657–1729), *Amphitheatrum Zootomicum*, Frankfurt, 1720, fol.

[2] Bernard Siegfried Albinus (1697–1770), *Oratio, qua in veram viam, quae ad fabricae humani corporis cognitionem ducat*, Leyden, 1721, 4to. *Academicarum Annotationum*, Leyden, 1754, Lib. i, 4to.

[3] John Claude Adrian Helvétius (1685–1755), *Idée générale de l'oeconomie animale*, Paris, 1722, 8vo.

[4] William Stukeley (1687–1765), *Of the Spleen, its description and History*, London, 1723, fol.

sinister physician is also said to have possessed a collection of anatomical preparations, but, to quote Hutchinson, 'after his decease Mr. Christie's auction room bore abundant witness to the frivolity of his collections'. Nicolai [1] praises isinglass as an injection medium, but finds that it extravasates too readily, and is apt to fill cavities and leave the vessels empty. Isinglass is also recommended by Mauchart,[2] who again is the first to experiment with injections of plaster—previously recommended by Boyle. The desire to imitate Ruysch's preparations, and the difficulty of doing so owing to a lack of knowledge of his procedure, is referred to by Wagstaffe,[3] who reproaches Ruysch, then living, for concealing his methods. He refers to the futility of exhibiting injections as a mysterious curiosity, and not as a scientific invention available to all for the advancement of knowledge. 'Dr. Ruysch has given us several excellent and curious drawings of the *finest preparations* in the world; but we had certainly been more obliged to him, if he had communicated his observations on the manner of *preparing* them, and form'd from thence a noble, a just and a demonstrative *rationale* of the uses of the parts.' This lack of candour, however, continued to discredit the publications of anatomists, for much later, in 1784, Sheldon, when referring to the great improvement in the art of injection 'in late years', says that 'progress of the science has undoubtedly been much impeded by the mystery observed among anatomists, respecting the composition of their injections, and their method of dissecting, injecting, and preparing the different parts: a mystery which deserves the severest censure, and is unworthy of the character of a philosopher or a man'. He adds: 'I shall disclose, without reserve, whatever I am acquainted with on this head.' Jesse Foot, in 1794,[4] praises Sheldon for 'his unreserved discovery [disclosure] of the art of injecting', and endorses his disapproval of a proprietary anatomy. On the other hand, Drake, in a posthumous work,[5] explains and

1 Henricus Albertus Nicolai (1701–33), *De directione vasorum pro modificando sanguinis circulo*, Strassburg, 1725, 4to.

2 Burchard David Mauchart (1696–1752), *Programma Anatomicum de iniectionibus sic dictis anatomicis*, Tübingen, 1726, 4to.

3 William Wagstaffe (1685–1725), In Drake's *Anthropologia Nova*, London, 1727, 8vo, ed. 3, vol. i, p. xi.

4 Jesse Foot (1744–1826), *The Life of John Hunter*, London, 1794, 8vo.

5 James Drake (1667–1707), *Anthropologia Nova*, London, 1728, 8vo, Appendix, Plate 51.

figures a large injecting syringe holding twenty ounces. The intake is separate from the outflow, and both have cocks so that the syringe can be refilled without removing the nozzle from the vessel, as first practised by C. Bartholin. The outflow pipe is a flexible one of leather, and the nozzle is of brass or silver, and small enough to ' pass into the lacteal vessels and chyliferous ducts '. Lancisi,[1] by syringe injections of mercury, coloured water, and air, perpetuates the error of Vieussens, Verheyen and others that ' there are meanders, winding passages, and diverticula leading from the coronary veins into each of the four cavities of the heart, and that the blood makes use of these as outlets '. By means of these ' outlets ' Lancisi believes that the blood ' alternately goes in and out with a kind of ebb and flow '. An attempt at histological injection was made in 1728 by Price,[2] who studied the structure of the villi of the ox and filled the vessels with wax, but no results of interest or importance accrued. Weiss,[3] improving on the practice of Ruysch, adopts a method which was almost at once developed and standardized by Monro and Cassebohm. He injects the human body by immersing it for a long time in warm water, and then throwing in first coloured turpentine to fill the smaller vessels, and afterwards a wax medium to distend and seal the larger ones. ' A gross mixture of wax, tallow, and vermilion ' is used by Mortimer [4] to inject the arteries, and the same substance, but coloured with smalt, to fill the veins. This is one of the few references to a differential injection of arteries and veins since the method was first introduced by Duncan in 1678. A general description of wax injection is given by Thiesen,[5] and Trew [6] is an early experimenter with fluid plaster, which, however, he does not consider an efficient substitute for wax. He tried also the

[1] Giuseppe Maria Lancisi (1654–1720), *De motu cordis*, Rome, 1728, fol.

[2] Charles Price, ' Remarks on the Villi of the Stomack of Oxen,' *Phil. Trans.*, London, 1728, vol. xxxv, p. 532, 4to.

[3] Johannes Nicolaus Weiss (1702–1783), *Theses sistentes viscerum glandularum*, Altdorf, 1729, 4to ; *De structura venarum*, ibid., 1733, 4to ; *De aquae adminiculo in administratione anatomica*, ibid., 1733, 4to.

[4] Cromwell Mortimer (ob. 1752), ' Case of some uncommon anastomoses of the spermatic vessels in a Woman', *Philosophical Transactions*, London, 1730, vol. xxxvi, p. 373, 4to.

[5] Gottfried Thiesen (n. 1705), *De materia ceracea eiusque iniectione anatomica*, Königsberg, 1731, 4to.

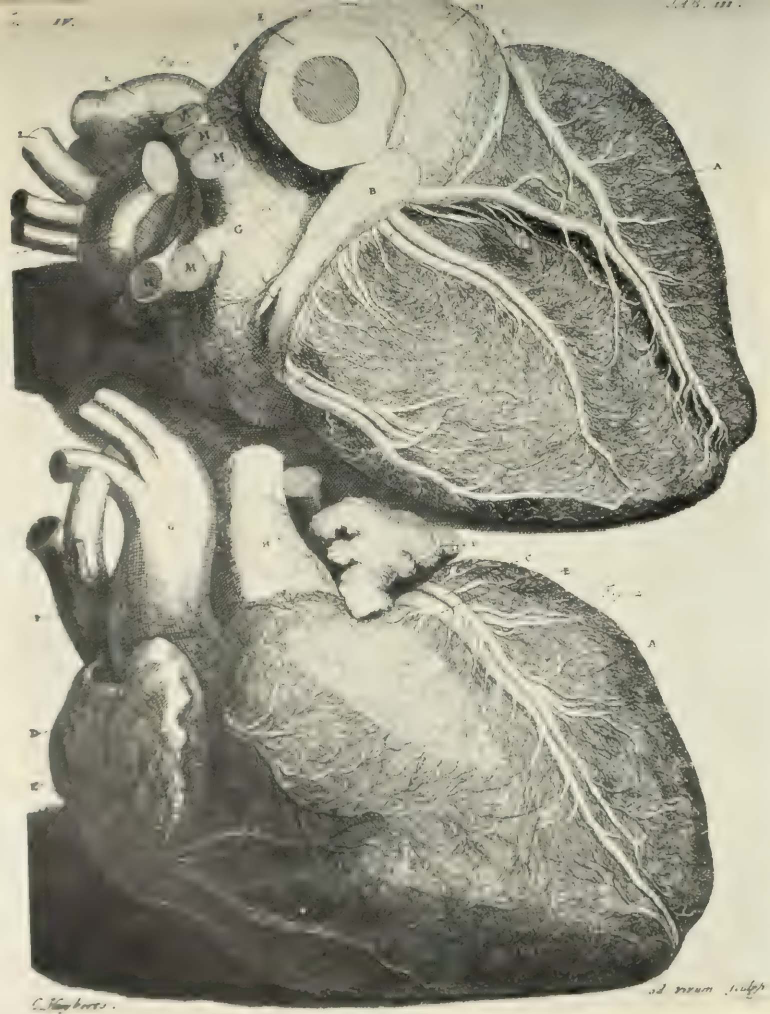
[6] Christopher Jacob Trew (1695–1769), *Commercium Litterarium*, Nürnberg, Spec. 9, 1731, Hebd. 30, 1736, 4to.

vegetable resins, resina anime and sandarach, and isinglass, and shows that injected mercury passes through the capillaries and completes the circulation. Vater [1] was a pupil of Ruysch, and was trained in injection methods by him. He employed different coloured liquids and wax, and according to Haller was most skilled in the art of filling the vessels, producing results equal to those of his master. The short paper published by Monro primus in 1732 [2] is important, not on account of its originality, but because of the influence it exercised, and the method there recommended was very widely adopted, and is not even now entirely abandoned. Monro's article, for example, forms the basis of the chapter on injection by Daubenton in Buffon's *Histoire naturelle.* Monro says: 'Scarce any anatomical books describe with accuracy the method of injecting,' and again, 'few have hit on the art of injecting the very small capillary tubes'. He uses a brass syringe which includes a contrivance for re-filling without removing the nozzle from the vessel—as previously recommended by Bartholin and Drake. For 'subtile or fine injections' he proceeds as follows: Macerate in warm water to liquefy the blood. First inject coloured oil of turpentine to fill the very small vessels, and follow up at once with a coarser injection to distend the larger ones. The two media mingle in the vessels, so that it is impossible to tell from inspection that two have been used. The colours employed are vermilion for arteries and distilled verdigris for veins—mixed with clear oil of turpentine and filtered or decanted to get rid of the granules. His coarse injection is composed of tallow, one pound, bleached white wax, five ounces, and salad oil, three ounces. Melt and add two ounces of Venetian turpentine. Sprinkle in the colour and filter through a linen cloth. Some, if not most, of the Hunterian preparations in the Royal College of Surgeons must have been injected by this method, since when they are cut or dissected they bleed from the finer vessels only. Two years later Monro published a striking but neglected paper [3] in which the independence of the maternal and foetal bloods in the placenta

[1] Abraham Vater (1684–1751), *De iniectionis variorum colorum utilitate ad viscerum structuram detegendam*, Wittemberg, 1731, 4to.

[2] Alexander Monro, *primus* (1697–1767), 'An Essay on the Art of injecting the vessels of animals', *Medical Essays and Observations published by a Society in Edinburgh*, Edinburgh, 1732, vol. i, 12mo.

[3] 'An Essay on the Nutrition of Foetuses', *Medical Essays*, &c., Edinburgh, 1734, vol. ii, 12mo. Some copies are dated 1733.



The coronary vessels injected by Ruysch (1704)


The spleen of the ox injected with wax by William Stukeley (1723)

is for the first time placed beyond question by injection experiments. On this occasion he makes use of oil of turpentine coloured with vermilion, and mercury. He says :

'The liquors are not carried from the mother to the foetus, or from the foetus to the mother by continued canals, that is, the uterine arteries and veins do not anastomose with the veins and arteries of the secundines ; but the extremities of the umbilical vein take up the liquors by absorption in the same way as the lacteal vessels do in the guts ; and the umbilical arteries pour their liquors into the large cavities of the sinuses, or other cavities analogous to them.'

It should, however, be pointed out that in Monro's time, to quote his own words, absorption was a process 'whereby the small open orifices of vessels imbibe liquors lodged in the cavities of the body', and hence the above passage is not as strictly accurate as it appears. Monro proves his case by injections of the human subject, and also of cows, sheep, and dogs. When the uterine arteries are fully injected none of the medium passes into the umbilical vessels or the foetus, though it was searched for most carefully. Conversely, if the umbilical arteries are injected, not one drop of the mass could be found in the uterine vessels.

'I have tried injections of very different kinds so often into the vessels of the womb and secundines of cows, prepared in all the different ways I could contrive for making liquors pass from the one to the other, without having once made a drop to pass, that I cannot be more certain of anything, than that there is no anastomosis or continuity of these vessels in cows.'

It is somewhat difficult to understand how Monro reconciled the results of his experiments with his definition of absorption just quoted. In 1736 Monro describes an attempt to inject the testis from the vas deferens with mercury,[1] but he only succeeded in partially filling the epididymis, the convoluted tubular nature of which and its communication with the vas deferens being thereby demonstrated.

Hales[2] is responsible for an ingenious and characteristic innovation. In order that the force of the injection might be known and kept constant, which cannot be the case when a syringe is used, he provided for the necessary pressure by using a known

[1] 'Remarks on the Spermatic Vessels and Scrotum, with its contents'. *Medical Essays*, &c., Edinburgh, 1736, vol. v, 12mo.

[2] Stephen Hales (1677–1761), *Haemastaticks*, London, 1733, 8vo.

column of the fluid to be injected, which column was maintained at such a level as to ensure a driving power equal to that of the arterial blood. Nevertheless, in spite of this precaution, extravasations occurred into the cavity of the gut. The apparatus consisted of a gun-barrel heated with boiling water, and having a brass cock at the bottom with which to regulate the height of the column. Sometimes two barrels were joined together, giving a height of ten feet. Hales confirms, what had previously been noticed, that vermilion is sometimes stopped by the capillaries, even when the vehicle of the injection passes into the veins. Before operating, the animal is heated with warm water, and occasionally Hales irrigates the vessels prior to throwing in the injection mass. The latter was either melted beeswax or a preparation which he attributes to 'Mr. Ranby' [John Ranby, the surgeon]. The latter medium consists of white resin and tallow, two parts of each, turpentine varnish, eight parts, and colouring matter (vermilion or indigo), three parts.

According to Cuvier, Nicholls [1] was celebrated for injections only inferior to those of Ruysch. In a manuscript copy of William Hunter's lecture notes in the possession of the writer, Nicholls is stated to favour coloured varnish as a fine injection. W. Hunter and Straus-Durckheim both assert that Nicholls was the first to make corroded preparations. We have seen that Bidloo, Cowper, and Homberg anticipated him by some forty years. The error is repeated in the *Dictionary of National Biography*, and Munk's Roll of the Royal College of Physicians. Kaau-Boerhaave [2] injected water into the cavity of the gut, and when the latter was compressed, the water was observed to pass into the veins and finally to wash the blood out of the portal vein. This experiment was held to prove that absorption was by the veins, and on the same grounds Duverney had previously concluded that the lacteal vessels were absent in birds. Kaau also studied how the lymph vessels might be made most conspicuous by injection.

Cassebohm,[3] who produced the first general treatise on

[1] Frank Nicholls (1699–1778), *Compendium Anatomico-Oeconomicum*, London, 1736, 4to.

[2] Abraham Kaau-Boerhaave (1715–58), *Perspiratio dicta Hippocrati per universum corpus anatòmice illustrata*, Leyden, 1738, 12mo.

[3] Johannes Friedrich Cassebohm (ob. 1743), *Methodus secandi et contemplandi corporis humani musculos*, Halla, 1740, 12mo.

anatomical methods after Lyser, devotes an interesting chapter to injections. He adopts the sound practice of experimenting with but few and simple methods, but their possibilities are exhaustively canvassed, and very detailed directions are given. Two types of media are tested: (1) non-coagulating fluids injected cold, such as coloured spirit, varnish, and mercury; (2) coagulating liquids injected warm, such as wax, suet, and isinglass. An ordinary thick medium is prepared from yellow or white wax thinned with deer or goat suet, lard, or turpentine according to the time of the year and consistency required. For fine injections isinglass is recommended. The colours selected are vermilion for arteries and verdigris for veins. A successful method was found to be first to inject paper or copal varnish coloured with vermilion to fill the smallest vessels, and to follow it immediately with a waxy medium similarly coloured to demonstrate the larger trunks. This agrees almost exactly with the procedure of Monro. Cassebohm made many attempts to wash out the blood from the vessels with warm water before injecting the solidifying medium, but he is only partially successful, and does not recommend it. As a subject he prefers a newly born child, or an abortion of seven or eight months with the umbilical string intact. The body is warmed with water before injecting, and in the case of the foetus the umbilical vein is filled first with the non-coagulating and then with the coagulating medium. Thus both arteries and veins may be injected in a single operation. His methods of injecting the adult body are complicated, e.g. his general injections are made from at least ten different points in the arteries and veins. Before filling the veins he endeavours to rupture the valves by means of an iron rod.

Le Cat[1] appears to have been the first to investigate the natural relations of a part by filling the cavities with a solidifying medium, and then preparing optical sections. This is also the essential principle of the paraffin section. Le Cat made use of melted resin and wax or very thick glue. A combined alcoholic and waxy fine injection, in which the human element is literally represented, is recommended by Fabricius,[2] but not elsewhere referred to. Its

[1] Claude Nicolas Le Cat (1700–68), 'On the figure of the canal of the Urethra', *Phil. Trans.*, London, 1741, 4to, vol. xli, p. 681.

[2] Philippus Conradus Fabricius (1714–74), *Idea anatomiae practicae*, Wetzlar, 1741, 8vo.

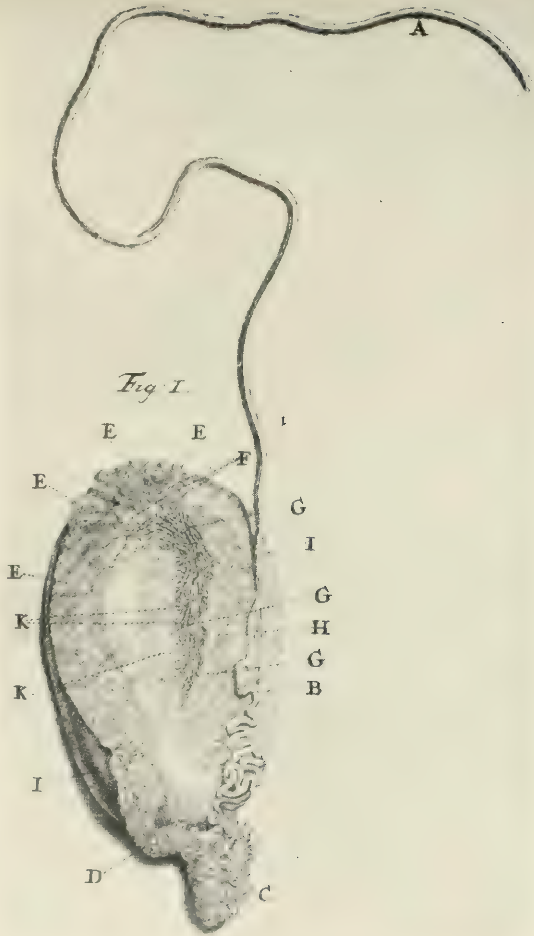
composition is: Rectified spirits of wine, 22 ounces; resin (sandarach), 2 ounces; resin (elemi), 1 ounce. Dissolve slowly over a sand bath, and then add: yellow wax, 2 ounces; human fat, 6 ounces. Colour with vermilion or verdigris worked up in alcohol. At about this time Westphal published a short tract on anatomical injections.[1]

An important experiment was described by Haller in 1745.[2] He injects the testis with mercury from the vas deferens so as to make its structure plainly visible to the naked eye. He doubts if it is possible to demonstrate this in any other way. An excellent figure is given, which is, however, too delicate to be reproduced except as a metal engraving.[3] In 1749 Haller sent an account of his experiment to the Royal Society which was printed in the *Philosophical Transactions* for 1750. 'Let the epididymis', he says, 'be gently and carefully filled with quicksilver, by the ductus deferens, now and then pausing, or dipping the testicle in warm water, that the vessels, being gradually expanded, may give way; for a sudden repletion will be apt to burst the middle or upper part of the epididymis. By this method, it has often appeared to me, that the epididymis, through its whole length by which it adheres to the testicle except the head, is one subtile canal, which is capable of being unfolded, as was perceived by de Graaf.' Haller also demonstrated the vasa efferentia and coni vasculosi, the rete vasculosum, the vasa recta, and the seminiferous tubules. All these structures had been already described, but less completely and accurately, by de Graaf in 1668, and some of them by Aristotle. A portion of the mercury had even penetrated into the seminiferous tubules, thereby establishing their tubular nature, and enabling Haller to determine the exact course of the seminal fluid on leaving the testis. He also discovered the vas aberrans. By further injections of wax and mercury Haller worked out the relations and structure of the vesiculae seminales. For general injections, however, Haller preferred the turpentine and vermilion medium to all others. It is difficult to understand, after reading Haller's paper, what was left for the Monros and the Hunters to

[1] Andreas Westphal (1720–88), *De iniectionibus anatomicis*, Greifswald, 1744, 4to.

[2] Baron Albrecht von Haller (1708–77), *De viis seminis*. Göttingen, 1745, 4to.

[3] This figure appears in Quain's *Anatomy*, but the beauty of the original is somewhat lacking.




Testis injected with mercury from the vas deferens by Albrecht von Haller (1745)

Micro-injection of the mucous membrane by Johann Nathanael Lieberkühn (1745)

dispute about as regards priority of the injection of the testis with mercury. The only advance they made on Haller's results was the quite minor one of filling more completely the seminiferous tubules. The success of the experiment of injecting the testis with mercury from the vas deferens may be gathered from the statement of Bowman in 1842 that 'there are not ten specimens that can be pronounced at all full in the Museums of Europe; and there is no evidence that, even in the best of these, the injected material has reached the very extremity of the tubes'. On the other hand, in the abstracts of the *Philosophical Transactions* published in 1809, it is stated: 'Beautiful specimens of the serpentine vessels or seminiferous tubes of the testicles, filled with quicksilver, are to be seen in the private anatomical museums of this metropolis.'

Lieberkühn [1] shares with Ruysch the honour of having made the most important contributions to the practice of anatomical injection. He carried Ruysch's methods a stage further, and is one of the first successfully to inject the *microscopic* vessels, and to make corroded preparations. His material was drawn mostly from the human subject, and he uses the same injection mass for the large and small vessels, except that for the finest branches it is further thinned with turpentine and the quantity of colouring matter increased. Sixty of his microscopical injections are still in the Museum of Human Anatomy at Vienna University. These preparations include the gills of the pike, salivary glands injected by the artery, vein, and excretory duct, periosteum and perichondrium, lung of frog, tortoise, ox and man, the villi of the intestine, and the placenta. Other injections are at the University of Berlin, where they are available for inspection. According to Adams, there were a few preparations by Lieberkühn in the British Museum. Some are at St. Petersburg, and were described by C. F. Burdach in 1817. A number were purchased by G. C. Beireis on the death of Lieberkühn's son, and are stated to be very beautiful and manifestly superior to Ruysch's preparations in St. Petersburg. Lieberkühn's injections were in fact so good that, a century after they were made, Henle was using them for

[1] Johann Nathanaël Lieberkühn (1711–56), *De Fabrica et Actione villorum intestinorum tenuium hominis*, Leyden, 1745, 4to. Some copies are dated 1744. 'Sur les moyens propres à découvrir la construction des viscères', *Mémoires de l'Académie Royale des Sciences*, Berlin, Ann. 1748, 4to.

the purposes of original research. He is the first injector whose sections will bear the highest magnification, and have undergone no deterioration since they were made. His injection apparatus was described and figured by J. C. F. Bonegarde in 1741, i.e. before the publication of Lieberkühn's work on the villi. J. C. Bohl also refers to Lieberkühn's experiments before 1745, so that his reputation as an injector therefore must have been established before the publication of his most famous work. The plates in this tract on the mucous membrane were etched by Pierre Lyonet—perhaps the greatest of anatomical engravers. Lyonet's beautiful plates of the Goat Moth larva were published in 1760, and his first attempts at copper-plate engraving, as he tells us, were made in 1743. Hence the plates in Lieberkühn's memoir must have been among the first efforts of the master.

In his 1745 paper Lieberkühn describes how he succeeded in injecting the arteries and veins of the villi of the intestine separately with different coloured wax. He first attempted a simultaneous injection from the artery and vein with two syringes, having nozzles adapted to the relative capacity of artery and vein. This resulted, however, in a mixing of the colours where they met. He then substituted injecting first the mesenteric artery with red, until by naked eye inspection some, but not all, of the villi were injected. Then he threw a green injection of lower melting-point into the mesenteric vein. Hence the two injections might meet but could not mix. Some of the villi were green and others red, and at the places where the red and green patches were in contact the arteries of the villi were red and the veins green. Thus was Lieberkühn the first to demonstrate microscopically the vascular supply of the mucous membrane. This had been injected before by Ruysch and others, but a microscopic examination of their preparations by Lieberkühn himself showed that they had not succeeded in injecting the capillaries either completely or without rupture. But Lieberkühn's injection and inflation experiments led him to perpetuate one serious error. By pushing the injection too far, the mass escaped into the cavity of the gut. He thus concluded, and his views were accepted almost without question for some time after, that the blood-vessels of the villi communicated with the lacteal vessels, and that the latter opened directly into the lumen of the intestine, each villus having at its apex one or more orifices. This mistake was revived by Hewson, and even

as late as 1849 Robin states that some anatomists still believed in the existence of these orifices. Before Lieberkühn's time the injections of Ruysch and his pupils had encouraged the belief that both the lacteal and the mesenteric blood-vessels opened directly into the cavity of the gut by large apertures. For example, a solution of indigo in urine forced into the intestine between two ligatures was found to pass into the lacteal vessels.

Lieberkühn's method of making corroded preparations, published in 1748, was as follows: Take white wax free from grease, add a fifth part of resin, a tenth part of Venetian turpentine, and of vermilion as much as is necessary. Inject the large vessels with care. Put the injected part into strong nitric or sulphuric acid diluted with water. Leave in the acid until the organic parts are destroyed, wash in water, and the result is a cast of the cavities of the large vessels. As, however, such preparations were very fragile, he devised a method of producing casts of the vessels in silver. Make a corroded preparation as above, and embed it in a paste made of two parts plaster and one part pulverized tiles in water. When set burn out the wax by graduated heat, and pour in the molten silver. Put the mould into vinegar, which disintegrates the plaster, and allows of its easy removal. To make a surface preparation of the vessels, the part is injected with the finer medium, and the vascular network is developed by directing on to it a strong stream of water, thus removing the organic matter. This gives very beautiful pictures in relief of the capillary network.

The eminent comparative anatomist Daubenton, who wrote the chapter on injections in Buffon,[1] occupies himself chiefly with a discussion of Monro's paper, and discloses no methods of his own. He describes in some detail fifty-five injected preparations in the Royal Museum at Paris, but does not state how they were prepared. Sue, in his general treatise on preserving and injecting methods,[2] gives practical directions for the preparation of injection media, and Quellmaltz [3] adds palm oil to the list of recommended

[1] Louis Jean Marie Daubenton (1716–99), 'Description du Cabinet du Roi'. In Buffon's *Histoire naturelle*, Paris, 1749, 4to, T. iii, p. 133.

[2] Jean Joseph Sue (1710–92), *L'Anthropotomie ou l'art d'injecter*, Paris, 1749, 8vo.

[3] Samuel Theodor Quellmaltz (1696–1758), *De oleo palmae, materie iniectionibus anatomicis aptissima*, Leipzig, 1750, 4to.

masses. Dr. Sandys is said by several contemporary writers to have been the first to make injected preparations transparent with oil of turpentine (*c.* 1750), but this useful and beautiful method was practised by Swammerdam, whose procedure was described by Schrader in 1674, and later by Ruysch.

We now reach the period of the second Monro and of the Hunters, whose teaching activities and Museums, if not exhibiting great originality in the matter of injections, did more perhaps than anything else to stereotype the anatomical injection in England. In January 1753 Monro secundus[1] injected the tubuli testis from the vas deferens with mercury, and hence confirmed the precise connexion between these two structures already established by Haller. A preliminary description with figure was published in 1754, and the complete work appeared in October 1755. Also in 1755, and again in 1757, he describes injecting the lymphatics with mercury, and concluded that they arose from the lacunar membranes and cavities of the body, and not from the arteries, as was at the time believed. In 1785 Monro investigated the structure of the gills of the Skate by injections of distilled oil of turpentine coloured with vermilion. Another, and quite novel, experiment was the injection of the lateral line canal of the Skate with water, air, milk, mercury, and oil of turpentine and vermilion. He discovered the tubules which arise from the main canals and open on to the surface of the skin, but committed the serious error of concluding that the lateral line system was a part of the lymphatic apparatus. He states that in 1765 he injected the mesenteric arteries with red wax, the corresponding veins with yellow wax, and the lymphatics with quicksilver, but the description of what must have been one of the earliest triple injections was not published until 1770. In the meantime, in 1769, Hewson had figured an injection of the mesentery of the turtle, in which the arteries had been filled with red wax, the veins with black wax, and the lacteals with mercury. Nearly all the early injectors confined their operations to Mammals, and Monro's work is therefore doubly interesting because of its strong comparative bias. Thus he was the first to inject an Echinoderm. Mercury was led into the tube feet of a common sea urchin, and he was able to

[1] Alexander Monro, *secundus* (1733–1817), *Essays and Observations Physical and Literary*, Edinburgh, 1754, 8vo, vol. i; *De Testibus et Semine in variis animalibus*, Edinburgh, 1755, 8vo; *De Venis lymphaticis valvulosis*, Berlin, 1757, 8vo; *The structure and physiology of Fishes explained*, Edinburgh, 1785, fol.

demonstrate the connexion between the tube feet, ampullae, and radial water-vessel. The subject was not an easy one and he made mistakes, but certain facts were correctly elucidated. Again in the same animal he injects the vessels of the intestine with mercury, and from them 'filled a beautiful network of vessels, not only on the intestines, but dispersed on fine membranes, which tie the intestine to the inner side of the shell'. The injections of Echinoderms to be seen in the Museum of Anatomy, Edinburgh University, were probably made by Monro.

In 1752, William and John Hunter [1] injected the epididymis and seminiferous tubules of the testis with mercury from the vas deferens, thus repeating the experiment first demonstrated by Haller. This injection was described by William Hunter in his lectures, but no account of it was published until 1757, and then only a very brief one. From 1746 William Hunter taught, but again did not publish before 1757, that the lymphatics were independent of the arteries and veins. This view was based on injections of both systems, it being found that an injection mass would not pass from the blood-vessels to the lymphatics or vice versa, and either system could be completely injected without affecting the other. Only by forcing the injection and rupturing the vessels could other results be obtained. The blood vascular apparatus was therefore a closed system of tubes without any direct connexion with the lymphatics, as was in Hunter's time, and indeed for long after, generally accepted. The presence of numerous valves in the lymphatics supported Hunter's view that the motion of the lymph was independent of the driving power of the blood-stream. The lymphatics were held to commence blindly from the lacunar surfaces and interstices of the body.

In the description of his plates of the gravid uterus, published posthumously in 1794, William Hunter states that he first injected the vessels of the foetal placenta from the navel string in 1743, but it was only when the plates were first issued in 1774 that this experiment was described. He says that he injected the placenta with wax of different colours—the uterine arteries red and the veins blue, but none of the injection mass passed into the vessels of the navel string. In the 1794 publication further details are added. The placenta of man and quadrupeds, he remarks, is

[1] William Hunter (1718–83), *Critical Review*, London, 1757, 8vo; *Anatomia uteri humani gravidi*, Birmingham, 1774, fol.; *An Anatomical Description of the Human Gravid Uterus*, London, 1794, 4to.

composed of two parts intimately blended—a foetal element, which is the continuation of the umbilical vessels of the foetus, and a maternal, which is an 'efflorescence of the internal part of the uterus'. Injection experiments carried out on man and quadrupeds demonstrated that the maternal and foetal bloods were absolutely distinct in the placenta, and both systems might be injected 'to an amazing degree of minuteness' without the two masses mingling. Nevertheless, each mass completed the circulation of its respective part, and returned to the parental or foetal centre from which it originated. This hypothesis is, of course, an old one. It was originally suggested by G. C. Aranzio in 1564, denied by Dulaurens in 1598, Fabrici in 1600, and Noortwyk in 1743, reaffirmed by Harvey in 1651 and Needham in 1667, but was first clearly established by the injection experiments of Monro primus published in 1734, of which the subsequent work of William Hunter afforded ample confirmation. Harvey's reasoning on this point is remarkably sound, but it is apparent that he and the writers before him base their views not on observation but on probabilities. Indeed Fabrici, in supporting the view of the ancients, admits that he has no positive evidence to offer 'because the fleshy mass itself stands in the way of any accurate investigation'. When the second Monro and William Hunter were students, it was still believed that the maternal blood circulated through the foetus by the navel string, and returned to the parental vessels, in spite of the positive demonstration to the contrary by the first Monro. Hunter's belief in injection methods was deeply strengthened by his visit to Albinus in 1748, when the beautiful preparations of the Leyden Professor fired the imagination of the Scots anatomist.

William Hunter was teaching anatomy from 1746 to the year of his death in 1783. In an excellent manuscript transcript of his lectures in the writer's possession, unhappily without date, four lectures out of eighty-two are devoted to injection methods—a proportion large enough to emphasize the importance in which injections were held at the time. He states that nothing has contributed more to the promotion of anatomical discovery, and that 'there is no making a good practical anatomist without it'. His watery injections are made from glue, isinglass, or gum arabic, and for the finest injections he used turpentine thickened with a little resin. The directions given for preparing lead casts of the

vascular and other cavities of the body are too similar to those published by Lieberkühn in 1748 to have been independently evolved by Hunter.

John Hunter,[1] in some notes written about 1770 but not published until 1861, contrary to modern practice, prefers stale material for injection purposes—even material which has been preserved in spirit. He soaks it in water until it has undergone a certain degree of putrefaction. This produces a relaxed condition of the vessels, and the blood is then cleared out by injections of warm water. His injection media are: resin and tallow; turpentine, hog's lard or tallow; hog's lard by itself or butter; glue or size; and isinglass. He notes that the finer the injection the more colour is wanted. The gravid uterus is injected simultaneously by the artery and the vein, so that the distribution of these vessels may be studied in the placenta. The colours selected are vermilion, King's yellow (a preparation of orpiment), blue verditer (a hydrated oxide of copper), and flake white. Green is made by combining yellow wax with the blue verditer. For corroded preparations, the material for which must be fresh and healthy, he recommends a vigorous injection of a mixture of wax, resin, turpentine varnish, and tallow.

After the Hunters, no important development in injection methods is to be recorded until the introduction of the 'soluble' form of Prussian blue, and carmine gelatine and other precipitates as colouring matters, in the nineteenth century. By the end of the eighteenth century the superiority of a solidifying gelatinous vehicle, such as isinglass or size, had, after many years of trial, at length asserted itself. Wax and varnish media were still employed for ordinary coarse and fine injections, but for injections of the capillaries intended to challenge the scrutiny of the microscope, gelatine was manifestly the most appropriate medium, and it is astonishing that recognition of this fact was so long delayed.

The remaining work of the second half of the eighteenth century may be briefly dismissed. Laghi [2] uses nut oil and thin glue or size. Lyonet,[3] the author of a great work on the anatomy of the

[1] John Hunter (1728–93), *Essays and Observations on Natural History*, London, 1861, 8vo.

[2] Thomas Laghi, 'De Iniectionibus', *De Bononiensi Scientiarum et Artium Instituto atque Academia Commentarii*, Bologna, 1757, 4to, T. iv.

[3] Pierre Lyonet (1707–89), *Traité anatomique de la Chenille*, La Haye, 1760, 4to.

Cossus larva, made little use of injection methods. He tried, however, injecting ink and coloured liquids into the heart of his larva. The results made him doubt whether the animal possessed blood-vessels at all, and he suspects that nutrition is effected by some means apart from the heart, for which another function must be sought. The connexion between arteries and veins by means of closed capillaries was confirmed by the injection experiments of Jancke,[1] but the work of the period is concerned rather with the lymphatic system, for the injection of which mercury was still the most popular medium. Hewson[2] states that the walls of the lymphatics, though thin, are strong, and will withstand a higher column of mercury than the blood-vessels. The lymphatics of fish are injected with mercury or thin coloured size from the ventral or abdominal lymphatic trunk, from which the medium passes into the entire lymphatic system. His experiments support the doctrine of the independence of the lymphatics, and, apart from extravasations, he holds that there is no connexion between them and the blood-vessels. Meckel's mercury injections[3] of the lymphatic and mammary glands and other organs resulted in the unfortunate conclusion that the veins are directly connected with the cavities of the tissues and thus absorb from them—a result at once questioned by Hewson. A new use of an old substance of unknown composition called cera punica or Punic wax was initiated by the injections of Walter.[4] His museum included a large number of beautifully injected preparations, but these apparently were injected with the usual red, yellow, and green wax. According to Hyrtl, Punic wax was soluble equally well in oil, spirit, and water, and combined readily with quicksilver. Wornum believes that it is prepared by boiling common yellow wax three times with sea water, to which a small quantity of potassium nitrate has been added. It would be difficult to

[1] Johannes Gottfried Jancke (1724–63), *De Ratione venas angustiores imprimis cutaneas ostendendi*, Leipzig, 1762, 4to.

[2] William Hewson (1739–74), 'On the Lymphatic System in Birds', 'On the Lymphatic System in Amphibious Animals', *Philosophical Transactions*, London, 1768–9, 4to, vols. lviii and lix, pp. 217 and 198; *A Description of the Lymphatic System in the Human Subject*, London, 1774, 8vo.

[3] Johann Friedrich Meckel (1724–74), *Nova experimenta et observationes de finibus venarum*, Berlin, 1772, 8vo.

[4] Joannes Gottlieb Walter (1734–1818), *Observationes Anatomicae*, Berlin, 1775, fol.

PLATE XLVI

General scheme of the lymphatics of the human body based on mercury injections by William Cumberland Cruikshank and his pupils (1786)

decide which statement discloses a greater ignorance of the actual constitution of the substance in question. The first author to devise a method of injecting the uriniferous tubules of the kidney was Schumlansky.[1] This was done by forcing the injection into the arteries until the glomeruli were ruptured, thus permitting the injection to extravasate into the tubules, and to pass from thence into the pelvis of the kidney. The method was afterwards successfully practised by Bowman in 1842. Ruysch had accidentally produced the same result before, but he considered the passage of the mass from the arteries to the tubules to be effected by natural channels. Schumlansky was also the first to hold that the Malpighian body not only secreted the urine but constituted the origin of the uriniferous tubule, which therefore was the means by which the secretion was conveyed to the ureter. Further, with the assistance of a vacuum pump, he inflated the uriniferous tubules until the air reached and filled the tubules in the cortex of the kidney. Sheldon[2] considers that the lymphatics can seldom be injected by any other medium than quicksilver, except the thoracic duct, which he fills with a coloured 'coarse injection' compounded of yellow resin, pure mutton suet, and wax. He was the first to shape the end of the canula as in the modern hypodermic syringe in order to facilitate its entry into the vessels, and he recommends drying injected specimens and afterwards making them transparent with turpentine. Full directions are given for mercury injections. Faujas[3] prints an interesting

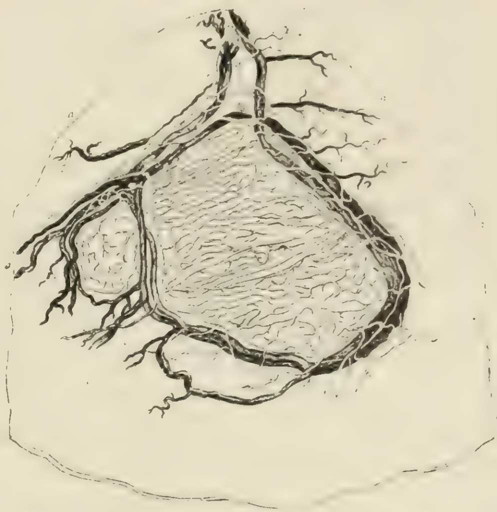


FIG. 8. Triple injection of the arteries, veins, and lacteals of the mesentery of the turtle by William Hewson (1769).

[1] Alexander Schumlansky, *De Structura Renum*, Strasbourg, 1782, 4to.

[2] John Sheldon (1752–1808), *The History of the Absorbent System*, London, 1784, fol.

[3] Barthélemi Faujas de Saint-Fond (1741–1819), *Voyage en Angleterre, en Écosse, et aux Îles Hébrides*, Paris, 1797, 2 vols., 8vo.

and familiar account of an interview with Sheldon, who communicated to him his method of preparing mummies by injection. The body was injected at intervals with strong spirit saturated with camphor and diluted with a little turpentine. The skin was well rubbed with finely powdered alum, and a flesh tint imitated by a coloured preparation being thrown into the carotid artery. A varnish composed of powdered camphor and common resin was found to preserve excellently the soft parts of the body. A final injection of the alcoholic solution of camphor by the crural artery completed the process, and the body was then protected from the air by a double case of timber. The entire corpus of a young woman so preserved, after the lapse of many years showed no signs of decay, the arms remaining flexible and the flesh throughout almost as supple and elastic as in life. This specimen is now in the Museum of the Royal College of Surgeons.

There are injections preserved in the Museum of Human Anatomy at Vienna, prepared by Joseph Barth (1745–1818), said to be quite equal to those made by Lieberkühn. Barth used wax, mastic varnish, and a small quantity of fatty oil, to which a lavish proportion of vermilion was added. The English anatomist Cruikshank,[1] who enjoyed the patronage of Dr. Johnson and achieved the doubtful distinction of being referred to by De Quincey in his Murder Essays, has his own method of injecting the lymphatics. He first injects the arteries and veins of the part in question, and also the excretory duct if a gland—the object being to fill all the cavities which are not lymphatic. He then throws the preparation into water and allows it partly to putrefy. Gas collects in the only vessels uninjected, i.e. in the lymphatics, which therefore become visible. They can now be punctured, the air forced out, and filled with mercury. The work on the lymphatics by Mascagni,[2] published in 1787 but dating some years earlier, surpasses all other efforts of a similar nature, to whatever period they belong. He filled the lymphatics with mercury, and counter-injected the blood-vessels chiefly with glue and vermilion. He points out that the particles of the vermilion are only slightly larger than blood corpuscles. He tried also

[1] William Cumberland Cruikshank (1745–1800), *The Anatomy of the Absorbing Vessels of the Human Body*, London, 1786, 4to. Second edition, 1790, 4to.

[2] Paolo Mascagni (1752–1815), *Vasorum Lymphaticorum Corporis Humani Historia*, Senis, 1787, fol.

tallow, wax, and plaster. The mercury was introduced as usual by a gravity tube, in this case of glass, having two arms at right angles to each other. The vertical arm was the larger and contained the mercury, the horizontal arm being sufficiently tenuous to be introduced into the finest vessel. Poli [1] injected Lamellibranchs with mercury. The injection was pressed too hard and penetrated into the visceral ganglion, from thence forcing its way along the nerves radiating from the ganglion. Poli was hence led to conclude that the ganglion was a part of the lymphatic system and acted as a lacteal reservoir, the nerves corresponding with the lactiferous vessels. A novel and highly interesting paper was published in 1794 by Sir Anthony Carlisle.[2] He injected the excretory canals and reproductive organs of the cestode joint with coloured size, and was the first to work out the course of the former vessels, in which he correctly suspected the presence of valves. An excellent discussion of injection methods in use at the time, accompanied by practical directions, is given by Sir Charles Bell.[3] 'Injection', he says, 'has been the great instrument in the hands of modern anatomists.' His masses include preparations of varnish, wax, resin, tallow, size, oil, and turpentine. The colours are as usual, except that lamp-black is included. The general method is that of Monro—a thin non-coagulating medium followed by a stiff solidifying mass for the larger vessels. Bones are injected, decalcified, dried, and finally made transparent with turpentine. Pole's 'Instructor' has evidently been drawn upon.

The first general treatise in English on anatomical injection methods and technique was produced by Pole in 1790.[4] No original researches are embodied in the work, which is a comprehensive and thoroughly practical account of what was known at the end of the eighteenth century. The English Lyser of 1740 does not deal with injections. Four types of injection media are described—coarse (seven formulae), fine (six formulae), minute (six formulae), and mercurial. A cold injection, which sets after

[1] Giuseppe Saverio Poli (1746–1825), *Testacea Utriusque Siciliae*, Parma, 1791, fol.

[2] Sir Anthony Carlisle (1768–1840), *Transactions of the Linnean Society*, London, 1794, 4to, vol. ii.

[3] Sir Charles Bell (1774–1842), *A System of Dissections, explaining the Anatomy of the Human Body*, Edinburgh, 1798, fol.

[4] Thomas Pole (1753–1829), *The Anatomical Instructor*, London, 1790, 12mo.

some hours, is added on the authority of William Hunter. The coarse injections are preparations of wax: the fine, of varnish; and the minute, of gelatine. According to Pole, mercury was going out of fashion, and was seldom employed when other masses were available. It is significant of the expense of spirit and glass that most preparations are recommended to be dried and varnished.[1] Vermilion injections are directed to be preserved face downwards, so that when the colour settles in the vessels it will be on the side that is seen. There is no mention of fusible metal in the section on corroded preparations. Another text-book on practical anatomy was published by Hooper in 1798.[2] Its inspiration, however, is obviously derived from Pole, than which it is also less complete.

Between the opening of the nineteenth century and the inception of the modern period there is little of importance to record. In 1819 C. A. Rudolphi injected the digestive system of the Liver Fluke with mercury. Huschke[3] injected the uriniferous tubules of the kidney. The surface of the kidney was exposed to the action of a vacuum pump, and an injection of size and vermilion was thrown into the ureter. It penetrated along the tubules as far as the surface of the kidney, but the operation was not generally successful. Huschke saw the artery enter and leave the glomerulus in the Salamander, but denied that the glomerulus was continuous with the uriniferous tubule. Injections of Vertebrates other than mammals, and Invertebrates, were so much the exception that it is interesting to note that, as catalogued by Alessandrini,[4] such a collection was instituted in the University of Bologna in 1808. Fohmann[5] was the first to inject the lymphatics by random punctures in places where they form a network. This is the so-called ‘ponction réticulaire’, as distinguished from ‘ponction directe’, where the syringe is inserted into a specific and recognizable vessel. The method was revived by Rolleston in his

1 Cf. Cole, ‘History of the Anatomical Museum’, *Mackay Miscellany*, Liverpool, 1914, 8vo, p. 304.

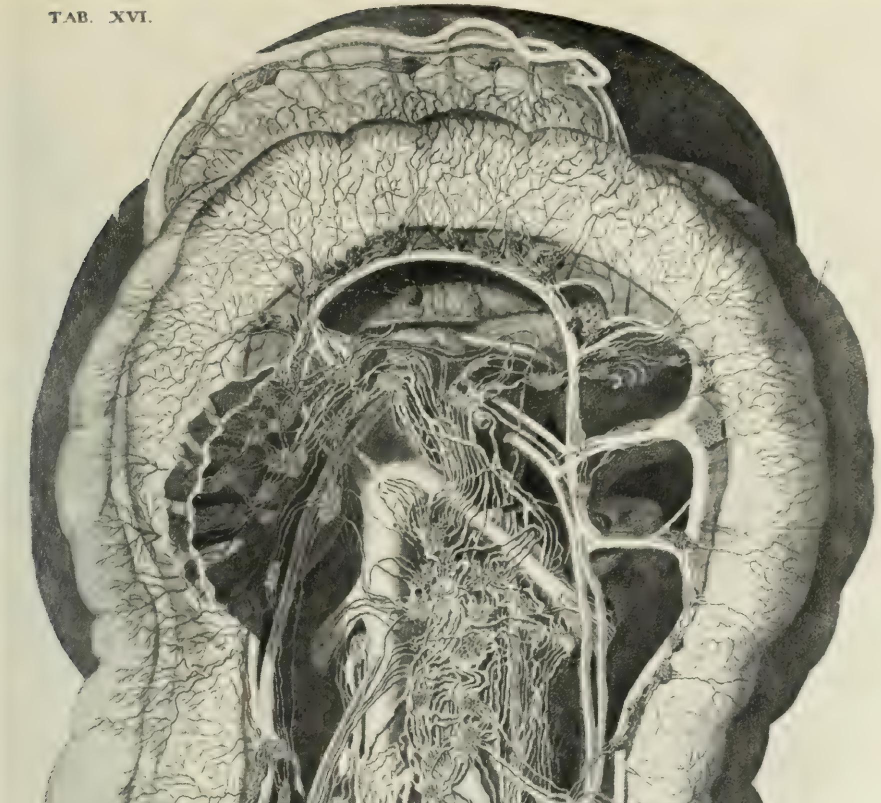
2 Robert Hooper (1773–1835), *The Anatomist's Vade-Mecum*, London, 1798, 8vo.

3 Emil Huschke (1796–1883), ‘Ueber die Textur der Nieren’, *Oken, Isis*. Jena, 1828, Bd. xxi, col. 560–72.

4 Antonio Alessandrini (1786–1861), *Catalogo del Gabinetto d'Anatomia comparata della Pontificia Università di Bologna*, Bologna, 1854, 8vo.

5 Vincenz Fohmann (1794–1837), *Mémoire sur les vaisseaux lymphatiques de la Peau*, Liége, 1833, 4to.

PLATE XLVII




Mercury injection of the lymphatics of the human colon and abdomen
by Paolo Mascagni (1787)

Harveian oration of 1873, and is still extensively practised. The expression 'double injection' was usually understood as referring to an injection first of a fine and then of a coarse medium, as practised by Monro; but it was sometimes used to distinguish the experiment of Doyère and Quatrefages.[1] These authors injected a dog successively with two different solutions which met in the vessels and there gave rise to a precipitate. Two of the substances employed were potassium chromate and acetate of lead, the result being a yellow precipitate of chromate of lead. In spite of the fact that this method was successfully exploited and spoken highly of by Bowman in 1842, it has never been developed, and indeed did not survive the criticisms directed against it by Robin in 1849.

A general and important review of injection methods, based largely on first-hand investigation, was published by the distinguished comparative anatomist Straus-Durckheim in 1843,[2] and the issue of this work may be considered to mark the termination of the historical period covered by this article. Straus-Durckheim, who speaks frequently of the expense of the materials employed, classifies injection masses into three groups: (1) coarse injections, of which fifteen are described; (2) fine injections, nine described; (3) corrosion injections, seven described. The substances tested are many and various. They include: yellow or white wax, tallow, lard, spermaceti, fatty oils, essential oils especially of turpentine and lavender, Venetian turpentine, Burgundy resin, hard resin, plaster, gelatine, white of egg, milk, water, alcohol, mercury, and fusible metal. He recommends spermaceti mixed with an essential oil because it penetrates the best, solidifies well, is transparent, takes a very brilliant colour, and any extravasations are easily removable. He has a cold solidifying injection mass made by the action of nitric acid on olive oil, but it does not appear to be very satisfactory. He finds that the setting of plaster masses may be delayed by the addition of gelatine. Gelatine he considers the best material for fine injections, and the variety of it he uses is isinglass. White of egg is diluted with water and coagulated with ferric sulphate—a procedure which has much to recommend it. Straus-Durckheim

[1] Louis Doyère (1811–63) and Armand de Quatrefages (1810–92), 'Sur les capillaires sanguins', *Extraits des Procès-Verbaux des Séances de la Société Philomatique*, Paris, 1841, 8vo, p. 17.

[2] Hercule Eugène Straus-Durckheim (1790–1865), *Traité pratique et théorique d'anatomie comparative*, Paris, 1843, 8vo.

regards mercury as one of the worst injections it is possible to employ—an opinion which had been gaining ground before his time, and became general soon after. His fusible metal for corroded preparations is made of bismuth, eight parts; lead, five; and tin, three parts. A small addition of mercury further reduces the melting-point. He experimented with the following colours: vermilion, carmine, indigo, Prussian blue, gamboge, verdigris,

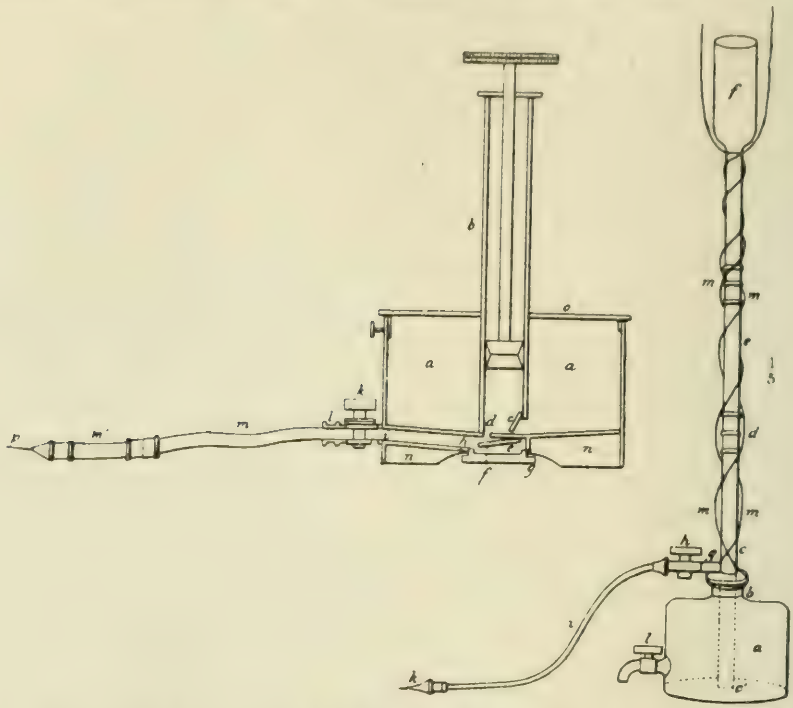


FIG. 9. APPARATUS DESIGNED BY HERCULE EUGÈNE STRAUS DURCKHEIM (1843).

In the top figure the syringe can be recharged without disconnecting any part of the apparatus. In the lower figure the necessary pressure is obtained by the gravitation of the mercury.

lamp-black, madder, 'orcanète' (a vegetable red dye extracted from the roots and bark of Anchusa), 'orseille' (a reddish purple extract of lichens), chrome yellow, Indian yellow, and neutral Potassium chromate. As regards instruments, for small injections the syringe of Swammerdam is satisfactory, but he prefers a more complicated injection pump which enables a considerable body of liquid to be thrown in without removing the canula from the blood-vessel. The figure (9) explains itself. Other simpler contrivances are described for injecting small animals such as Mollusca

and Crustacea. For fine injections, and especially for the lymphatics, he devised an apparatus called the injector, which he himself had used for eighteen years (Fig. 9). It consists of the vessel *a* filled with the injection mass, which passes by the outlet *g* to the canula. The vertical tube *c* may be varied in height, and extends almost to the bottom of the vessel *a*. If now mercury be introduced by *c*, it accumulates at the bottom of *a* and forces the injection mass out by *g*, the actual pressure depending on the height of the column of mercury selected. This apparatus was afterwards modified by Robin. Straus-Durckheim also describes methods of inflation, of exhausting the vessels with a vacuum pump before injecting, and of filling the vessels of very large animals such as the rhinoceros and elephant.

# SCIENCE AND THE UNITY OF MANKIND

By F. S. Marvin

This volume appears at a moment in world-history which will always be recalled for criticism or for admiration, probably for much of both. After a time of devastation and loss throughout the world unparalleled in history, a supreme attempt has just been made to combine mankind in a working union to prevent future conflicts and promote the peaceful solution of the international differences of the future. This is the greatest political fact of the moment. And in the sphere of knowledge science has reached the point at which its volume and complexity, its necessity for life and for education, are being at last fully realized in all civilized countries. There are thus two maxima before us, one in the political, the other in the intellectual world. We are in these studies interested in science, and especially in its history. But it would be a fatal mutilation of the history of science to attempt to treat it without regard to the accompanying social and political conditions of mankind. Whichever is root or trunk or flower in the tree of progress, the whole is vitally connected, and we are therefore bound as historians of science to ask what are the relations between our two maxima ; can we trace any direct relation between the growth of scientific knowledge and the unification of the world which has led to this new expression in the League of Nations.

The inquiry is large and difficult enough. The war seemed for the moment the heaviest blow which the cause of unity had ever borne. Yet it was the war which has given birth to the League of Nations. The war diverted, too, some of the best brains from the study of science, and has laid many of them to rest for ever. Yet it was to the war that we owe the recent triumphs of aviation, besides a multitude of minor inventions which may have their use in peace. Neither optimist indeed, nor cynic, can hold his own in this debate, and we shall be content with very wide and suggestive rather than demonstrated conclusions.

There is first the abstract question, the essentially social nature of science itself. It must be noted how science arises in the active

mingling of the thought of many minds, and at times and places where men of various race and antecedents have met and interchanged ideas. Then comes the historical, or *a posteriori*, inquiry, how far the growth of science has been accompanied by a closer knitting up of the world as one community. This may be shown in a growing intimacy of all sorts of relations, in politics, commerce, culture, and religion. And the historical argument will lead finally to some consideration of the actual links which science has forged for the process of unification, and what it may do in the future to hasten it.

First, then, the abstract question, how far does the nature of science itself point to unity in mankind ? Science, i.e. organized or connected knowledge, is a social product. It made its appearance after communities of men on a fairly large scale had already been formed, and it was both the result and the means of a widening intercourse and communion between them. This fact, which is clear historically, is in no contradiction with the equally obvious fact that science in a rudimentary sense is present in the simplest inference by which the savage, or the animal for that matter, guides his action by the observation of certain sequences in the external world. The storm threatens, the plant poisons, the fire burns, and the most elementary of minds frames its conduct accordingly. 'All men', says Aristotle at the opening of the *Metaphysics*, 'desire to know', and it is by this differentia of conscious knowledge that the species is defined. But at the outset it was the necessities of life and not only or even mainly man's intellectual curiosity which prompted the forward process. By his quicker wits, his better memory, his greater readiness to fit means to ends, man was enabled to endure the trials of nature and to gain the mastery over the other animals, many of them far stronger physically than himself. Now all these desirable qualities are elements in the structure of science. Yet it is necessary for clearness of definition to limit our terms. There is a point at which man's growing knowledge and experience take on a distinctive character and make a fresh start, and this point was the birth of Greek philosophy in Ionia. It concurs—and the concurrence is significant—with the intense social activity which centred on the western sea-board of Asia Minor in the seventh and eighth centuries B.C. Here was the meeting-place of travellers and trade-routes both from the eastern empires of Assyria and

Babylon, and from the south and south-east, Egypt, Phoenicia, and Crete. Here the quick-witted Greek was in touch with all that could be reported by word of mouth from the centres of old priestly learning, here he could meet men who had seen the greatest wonders of man's activity on earth, and could set out himself to explore and to question the thinkers and the workers who could describe these things on the spot. Hence the figure of Thales and of those early Sages of Ionia must be regarded not only as characteristic types of the Hellenic race, but as the recipients and transmuters of wisdom and observations long anterior to themselves—the mouthpieces of a social product to which the toiling millions of the Nile valley and the Euphrates, the royal owners of wealth and authority, the priestly houses of record and study, had all contributed their part. And they again rest on the more primitive cultivators of the same soil in ages of stone. This social connexion is of the essence of science, and it spreads far, not only in space—as we see in this first appearance in the Middle East—but in time also. There is affiliation with the past as well as widespread intercourse in the present. Science was not born until these two conditions had been fulfilled, and to them it owes the special intensity of its social character, purer even than the social nature of language or religion or art. It is the sociality of reason itself, the 'connected experience' which, as Aristotle says in the same passage of the *Metaphysics*, differentiates the reason of man from that of the other animals. 'Of all the mental treasures of the race scientific truth alone compels general acquiescence.' [1] To it alone the differences of race or age or nationality are indifferent. In it alone we see the complete fusion of mind with mind which constitutes 'sociality'. And through the steady spread of this general acquiescence, or communion in truth, over all obstacles of ignorance or prejudices or remoteness on earth, the growth of humanity is best exhibited. Just as in Aristotle's view the 'desire to know' is the differentia of man from the other animals, so in the spread and acceptance of science we have the groundwork of humanity, the progressive Being *par excellence* known to us in the world.

It is interesting and significant that the perception of the universal nature, the sociality of reason, was, like science itself, only gradually acquired. It has kept pace through the ages with

[1] Sir Wm. Osler's Harveian Oration of 1906, Clarendon Press.

the growth of science, and is only in these latter days fully accepted, with the full acceptance of the dominating voice of science. To the classical Greek the light of truth appeared to be only granted to a small *élite* of mankind. Round the little island of right-thinking and enlightened men was a great ocean of barbarian outsiders, perverse in their mental processes and language, unintelligible to those who were on the way to truth. The Stoics, who were the universalists of the ancient world, made all men equal and all accessible to the dictates of reason. But they attained this position rather by evacuating reason of the scientific content which it had acquired during the Greek evolution than by seeking in all the humblest manifestations of reason the germs of the fuller growth. It was the universalism of Tolstoi rather than of Comte, though the Greek generalizing mind had made the premature attempt possible. Something of the same sort is true about the mediaeval theory, profoundly and impressively though Dante puts it. He tells us,[1] as Aristotle did, that the height of human power, the quality of man that makes him man, is thought, the power of understanding things. And he goes further than Aristotle towards the social source of knowledge. He is explicit that this endowment of reason is not an individual thing ; no one can thus think by himself. It belongs to man as a species, and only by the multitude of other men can any one man enjoy his faculty or increase it. On this side, then, Dante goes further and deeper than any one had gone before, for the universalism of Christianity was behind him and inspired his thought. But on the side of the nature of science he shared the arrested development inherent in the mediaeval position. These multitudes of men from whom the individual derived his reason and with whom he should enjoy it, were not co-operating agents in an infinite process of building up truth, but humble participants in a feast already provided by divine wisdom for those who had grace to hear the invitation and a pure heart to partake of the bounties offered. It is not till the re-birth of science in the sixteenth and seventeenth centuries that we can see both the social nature and the infinite progressiveness of knowledge first dawning together on the Western mind. 'Mind begets mind', said William Harvey, in a pregnant phrase, at the beginning of the seventeenth century, and it is in that century that we can first trace clearly the two

[1] *De Monarchia*.

connected, and hereafter dominating thoughts, of 'progress' and 'humanity', both based on the growth of a collective mind, exhibited in its most articulate form in the conclusions, constantly verified and constantly modified, of scientific truth. We cannot stay here to watch the gradual expansion of these ideas ; but one feature in the nineteenth century demands some notice as perhaps the greatest single contribution to the conception of science as a social product. This is the contribution from archaeology. It was archaeology which established the notion of a firm, converging tendency in human thought towards common conclusions about ourselves and the world we live in. It has dispelled the barriers which seemed both to the Greeks and to the Catholic mediaevalists to divide mankind. It shows us the whole of our species struggling together from a common starting-point, by very similar steps to a common goal. The story thus unfolded is no more an idyll of brotherly love than any other part of history. It must indeed have been far less so, difficult though that may be to believe. Multitudes of races, as of individuals, fell by the way, more perhaps by the hands of fellow men than by other enemies. And yet the real progress which was achieved was by extended co-operation. The directing mind was social in its origin and social in its purpose, conscious or unavowed. And in the light of archaeology we can see that the same social processes of thought were at work wherever our first fathers set up a totem to protect their tribe, or sacrificed to the divinity of an ancestor or a nature-spirit, or devised some machine or word to make the common work more effective or the common emotion more intense.

This theory of the sociality of thought which Durkheim worked out fully, and perhaps too exclusively, in the sphere of religion, might, we think, be pressed further home than it has been at every stage. It would then appear that science, in the systematic sense, is the strongest of all links both between individuals and between groups of men. The friends of science must, on this subject, part company from emotionalists of the Tolstoi type. It is true that, either for association or disruption, emotion is for the moment a far more effective force. It is simpler, more obvious, more accessible to the masses of mankind. But the links of reason persist and spread, they go back to the distant past for those who can follow them, and while capable of indefinite reinforcement by emotion, they can maintain their hold and are a force in them-

selves, independent either of support or assault by passion. In this sense it is true that science is the fundamental bond of our race.

But we must turn from the difficulties of a summary appreciation of the most complicated of psychological questions to the clearer issues which are presented on the historical side. Here we may see—if we start on a backward journey from the present into the past—that the world is now more of a unity than it has ever been, and that science is at a maximum. These are firm starting-points, and as we look back, we may trace some three or four great moments or pre-eminent steps in the advance of knowledge linked up with the unification of the world. The earliest, the first occasion on which we can speak of science in the stricter sense, is the Greek construction of science and philosophy which extends from Thales and Pythagoras in the fifth and sixth century B. C. to Ptolemy, the continuer of Hipparchus, in the second century A. D. Connected with that is the formation of the Greco-Roman world at the head of which were men trained in Greek philosophy and attempting to apply the rules of reason to the congeries of human facts which had come together in their hands. The second would be the revival of science in the fifteenth and sixteenth centuries, based on the recovery of the work of the Greeks. Connected with that on the practical side is the discovery of the New World and the expansion of the West. Then, towards the end of the eighteenth century, begins the last and most rapid movement of all: it may be dated from the invention of the steam-engine. In this period, the last hundred and fifty years, the progress on both of our parallel lines has been unexampled. An increase in scientific knowledge, a new linking up of the world, have taken place, which completely eclipse in volume all that the earlier centuries have done of the same kind.

Our historical argument is to review very briefly these great stages and point out a few of the salient facts which support the theory that science and unity are essentially connected.

When we look first at the Greek evolution, we are struck rather by the marked incapacity of Greek cities and Greek statesmen to achieve any form of permanent co-operation than by their tendency to unity. If the argument for the unifying power of science rested here, its foundations would be bad. But it is of course impossible thus to isolate the prominent contesting Greek states from their environment, Athens from Alexander, or on the

larger field, Greece from Rome. We should not be inclined to do so at all, were it not for the vicious tendency of the orthodox teaching of history which concentrates attention on the activities of Cleon and makes no serious estimate, probably does not even mention, Pythagoras, Hippocrates, Archimedes, or Hipparchus. And when we see how in the last two centuries B. C. the work and thought of the Greeks mingled finally with that of the Romans, it is clear that we must treat the two as complementary, parts of one whole, the organization of the Western world on the lines of the first scientific synthesis. Roman law is in fact a close analogue of Greek science, and in itself scientific. The two peoples, common in their racial origin, closely associated in the building up of city-state, were finally in the Roman Empire of the second century socially and politically merged ; for the Empire rested intellectually as much on Greek thought as it did practically on Roman roads and legions and municipia.

It may be said that in this very vague and general way every form of orderly government might be called scientific. No doubt there is force in this criticism, and if we could follow it out, we should find that the impulse to seek orderly sequence in external nature, and to apply consistent rules to the management of men, are indeed near akin. But we may make some rather closer applications of Greek scientific notions to public order and unification in the Greco-Roman world. Think of the influence of astronomy. Pliny tells us in one of his letters that the Greek science of the heavenly bodies had done much to allay the superstitious fears of the populations of Asia and to produce calm and order in the public mind. And when one remembers the primitive explanations offered of eclipses and the panics which they have caused among untutored peoples, one can well understand the change which must have come over the general mental attitude in the eastern Mediterranean, as the Greek precise and natural explanation of such phenomena made its way. But there is another astronomical conclusion which seems of even greater moment for the unification of the world and has not been generally treated of in that connexion. That is the sphericity of the earth and the first perception of the truth that our planet is one of the multitude of spherical bodies, suspended or pursuing a certain path in space. This was a Greek achievement, and to the deeper vision of history which is now opening up to us, it will appear

as one of the capital moments in human evolution. It seems to have been achieved by the time of Pythagoras. From that time on, but not before, it was possible for all the inhabitants of the earth to regard themselves as partners in one home with a common fate. Not till then could the ‘ humanum genus ’ of the Romans, the ‘ Dear City of Man ’ of the Stoics, begin to be envisaged with any reality. The subtle bias of such a thought towards the pacification and unity of the Roman world is obvious, though we cannot demonstrate its course of working. We may be as sure of it as we are of the world-wide desire at present for some effective confederation of mankind, though the conflicting policies of governments and newspapers will obliterate some of the approaches to the ideal for the future historian.

For us, reviewing now the approaches to the Greco-Roman system at its best, the main current seems to be the tradition of the Stoics and Cicero, down to men like the Elder and Younger Pliny, who were the backbone of the Empire. To Pliny the Elder Cicero ranked highest of all writers, save Homer alone, and Cicero was the man who, as quaestor in Sicily, showed his public spirit and his sympathy for Greek thought by rescuing the tomb of Archimedes from neglect. This type of Roman was no original thinker, not himself a contributor of any stones to the structure of science or philosophy, but being formed philosophically by Greece, and fully aware of the Greek superiority in abstract thought, he was able, in the Roman position of command, to spread ideas of intellectual as well as political order both East and West. East they spread more than West, for the area was already fertilized by Greek thought, and in that region the Greek tradition, in which a scientific habit of mind played its part, kept the Eastern Empire in being for a thousand years after the barbarians had broken up and resettled the Western. In the Catholic West, too, it was Greek thought which dominated and systematized the theology and philosophy of the Middle Ages, and thus formed again a link of unity.

But we must pass on to the re-birth of science in the stricter sense, when the men of the Renaissance brought their theories about nature into contact with the facts of observation and experiment. This was really the capital achievement of the Renaissance, outweighing the literary revival and the outburst of zeal for art, adventure, and enjoyment. The re-birth of science

is often represented as the throwing off of bad Greek traditions, and, so far as it consisted in abandoning the idolatrous following of the mistakes of Aristotle and Galen, this is true enough. Yet all the leading thinkers who began the new scientific movement of the sixteenth and seventeenth centuries were in contact with the Greek masters who had worked in the same field before, and made use of their results. So far as the earlier thinkers had thought scientifically, so far as truth grows as a whole, just as mankind is a whole, this filiation is clearly essential unless the entire work is to be done over again. Descartes and Galileo were just as much subject to the condition in mathematics and physics as were the schools of medicine and philosophy. It was Arab thinkers who, in many cases, had kept the torch alight.

But there is one point of contact which is of special interest here, both as it concerns the unification of the world, and as a striking example of the successful union between science and practice, which was to become increasingly fruitful in later years. We spoke of the doctrine of the sphericity of the earth, and saw that it had been accepted some time before the meridian of Greek thought. Aristotle clearly stated it, and it was worked out in detail by Ptolemy. The maps of Ptolemy, with latitude and longitude fairly correct for the parts of the world known to him, were forgotten in the West for a thousand years, and replaced by imaginary constructions based on the supposed teachings of Holy Writ. The sphericity of the earth was, in fact, formally denied by the Church, and the mind of Western man, so far as it moved in this matter at all, moved back to the old confused notion of a modulated ' flatland ', with the kingdoms of the world surrounding Jerusalem, the divinely chosen centre of the terrestrial disk. But at the beginning of the fifteenth century Ptolemy was recovered and translated into Latin, and by that time also intercourse with the Arabs and the journeys of Marco Polo and others to the East had prepared the way for a wider view. It was the re-establishment of the Greek doctrine of sphericity, the maps of Toscanelli based on Ptolemy, the currency which began again to be given to more scientific notions of geography, which inspired Columbus to seek the East by crossing the Atlantic. This is a salient instance of science stimulating the active powers of man to gain a completer knowledge of his earthly home, and through knowledge to bring it into one sphere of thought and action.

If we have an eye fixed on the ultimate goal of human federation, as we have in this essay, the expansion of the Western world which began at this time, the linking up of Europe with the Far West and the Far East by voyages of discovery, by commerce and by settlement, may well seem at least second if not first of all the achievements of the Renaissance, and ultimately far more important than the disruption of the Church and the rivalries of the new-born national states which absorb the larger part of the interest of the political historian of the period.

Another institution, also inspired by scientific thought, appeared at about this time, side by side with the Renaissance of physical science, even more potent perhaps in furthering the future unity of the world. This was Grotius's construction of international law, written in the first quarter of the seventeenth century, and contemporary with the work of Galileo and Harvey.

The analogy in the way of thinking between the first and greatest master of international law and the founders of physico-mathematical science is as close and striking as their coincidence in time. Both are clearly scientific. Just as in external nature thinkers from the time of the Greek Sages had been seeking laws, or forms, true for all geometrical figures of certain shapes, for all movement, celestial and terrestrial alike, so in Grotius a man arose who set out to ascertain what truths about man's nature in society might be assumed to be of universal validity and universal application. He was the first to look for these principles of universal right in man's own nature, apart from religious sanctions and superior to local fluctuations of time or race. On these universal principles he would build a universal code. Nothing obviously could be more consonant with, or more favourable to, our ideal of the unity of mankind. And this great effort appeared in the West at the same moment that the greatest of physical laws was being worked out by the labours of Galileo, and that Harvey was framing the first great mechanical law of physiology. Thus, if the statue of Grotius stands high, as it should stand, in the Courts of the League at Geneva, it should be recognized that the basis of his work, his own chief title to fame, rests in the application of scientific method to human affairs, and that this capital step was taken at the same time that modern science of the physical order was being launched on its triumphant career. Broadly considered, Grotius's work was part of the same move-

ment, the constitution of truth by human reason acting on the observation of facts and reducing them to general laws by induction tested by experiment. And if we are told that in the case of international law, the experiments of history have proved disastrous to the generalizations, we shall reply that in the case of any law of life, and above all of human life, time for realization is of the essence of the problem. The infractions of international law no more destroy it than occasional murders destroy the general prevalence of the law—if we may call it a law—of conjugal and filial affection. Grotius stands firm, and for his justification we do not need a record of unbroken conformity to his laws, but of a progressive acceptance and enforcement of them by the nations. History, even the history of the war, has fully established this, and Grotius's ideal is brighter than ever as a beacon for a world travelling towards peace and international justice.

We have to pass now, much too hastily, from the time of these first great essays of modern science to our own, or recent times, in which the youth of the seventeenth century, with all his vigour of invention and enthusiasm of faith, may seem to have come of age, stained with many faults, but vastly stronger and more competent and well-grounded in a knowledge both of his errors and his power. We are still tracing the unifying process, assisted by science, and we claim to find it in spite of the wars of religion in the sixteenth century, the wars of trade in the eighteenth, the wars with France and Napoleon at the Revolution, or the Great War which has just closed. This last might perhaps be called a scientific war in its methods and engines, but it can no more be attributed to the growth of science as its cause than the wars of the Revolution could be ascribed to Lavoisier or Watt's engine.

The steam-engine may indeed be taken as the turning-point in the history of modern science, from which its growth, steadily progressive from the seventeenth century, became markedly quickened, especially in its applications to life and above all in those applications which promoted the unification of the world. Apart from its subsequent uses in linking up the world, Watt's invention is a classical instance of the linking up of science and practice, theoretical training and industrial profit. For the inventor was a man of general scientific attainments, especially on the mathematical side, and he was working at Glasgow in a department for study and not in an ordinary workshop, when

Newcomen's engine was brought to him for repair and led to his capital improvements. And the decisive thought was suggested to him by another scientist, Black, the professor of chemistry in the university. The same story, of the immediate linking of abstract and disinterested study with practical applications, may be told of the other principal agent in improved communications in the nineteenth century, the electric telegraph. It derives directly from the researches of Oersted and others into the nature of electricity, just as the waves of Hertz fifty years later gave us wireless telegraphy.

It is enlightening, from many points of view, to examine closely such cases as these because they exhibit clearly the active thought of the West in contact with facts, giving increased power in practice, and forming fresh associations between men. The contrast with Eastern, especially Indian, thought is palpable, the thought which turns constantly inwards and gives one the constructions of mysticism, the ideals of Nirvana. It becomes an obvious truth, as these contrasts and these connexions are realized, that the unification of the world, proceeding from the nucleus forged by Greco-Roman thought round the Mediterranean, is essentially the creation of that form of orderly, organized, and objective thinking which we have called science.

We have been dealing rather with some of the material and mechanical expressions of the unification of thought than with the spiritual unity itself, though the latter must be regarded as fundamental, and is as certain as the former. The similarities of life induced by the spread of great industries, the rise of great cities, the connexions of railway, steamship, and telegraph all over the globe, are obvious to every globe-trotter, and are by no means an unmixed good. Side by side with the revival of regional associations and nationalist feelings and self-government has gone a general flattening out of ancient peculiarities, a diminution of separate languages, a dying down of secluded cults and customs. It is hard to strike the balance between the two tendencies, but on the whole the world seems, to most of those who have seen much of it in recent times, a more uniform and, some would say, 'vulgar' place than it used to be. This, so far as it is true, is one side of the process of unification, the depressing side. No birth takes place without some loss and suffering, and if a true birth of a spiritual kind is taking place—which our whole argument

tends to prove—then we may confidently hope that suitable forms and garments will follow to clothe the child of the future.

'It is no accident', Dr. A. N. Whitehead tells us in his recent book on the 'Organization of Thought', 'that an age of science has developed into an age of organization. Organized thought is the basis of organized action.' The words might have been taken as the text of this article. They sum up in the shortest and most unanswerable way the argument, that science being the 'organization of thought', the effort, and the result of the effort, to bring more and more facts of experience into an organic unity, must be, and has been, accompanied by a corresponding organization and unification of the beings who have produced that science. We have seen some of the external workings. It remains to look for a moment beneath the surface, and to consider how these spiritual forces may by cultivation, and with right direction from the general will, conduce to greater unity—of the desirable type—in future.

Social unity rests now, both nationally and internationally, in every unit large or small in which it exists, on the conscious co-operation of all the individuals who compose it. This is clearly and admittedly true in all civilized communities which have attained a national existence. It is true also of the large aggregates such as Russia, China, or India, which are struggling in various circumstances and difficulties to attain a conscious national existence. It was not the case in the great political aggregates of the past, the Roman Empire or the Holy Roman Empire of the Middle Ages. In these the unity was imposed from above. It is now to be attained from within, a task of incomparably greater difficulty and longer effort. But the new units, when organized, will have in them an enduring life of quite another order than the old. Within the family, the town, or the national state, besides the bonds of affection, there are the feelings of decency, of honour, of common interest, present more or less to the minds of all. Certain anti-social acts are never done except in moments of criminal excess; such acts, and others short of criminal, are reprobated and mostly checked by the social conscience of the community. Now, when we turn to the human community as a whole, we notice at once marked differences in the force and incidence of the various elements in the social conscience. Affection is far weaker: decency and honour are rather the reflections

of the smaller social conscience than of the human conscience as a whole : the common interest is hardly felt, when we are thinking of all humanity face to face with the unplumbed perils of the universe. But considerations of the more strictly intellectual order, of human dignity, of the solidarity of mind, of our debt to the past and the possibilities of the future, all these feelings, which are social also, are stronger on the wider basis of humanity than on the narrower. But they require more cultivation : they are present consciously to the minds of only a minority even in the best educated countries ; they seem to many of us to be unattainable by the mass. Yet they are essential to the unity of the future ; and science probably provides the most accessible channel for their entry to the mind. In no other branch of human progress is the advance so clearly demonstrable from the past, and in no other is it so obviously the joint work of all civilized nations, and even of the uncivilized, all co-operating according to their gifts.

The teaching of history in a new spirit will be one of the means, perhaps the most widely applicable, of deepening the intellectual basis of unity. If, as we believe, the League of Nations becomes shortly the dominant political fact in the world, the attention of all the associated peoples will be directed perforce to the character and history of their fellow members and the basis of the union between them. As soon as this question is raised, we come to science in the various senses and manifestations which have been alluded to in this paper. Hence any teaching of history which may be favoured by the League—and there are good reasons why it should interest itself in the matter and give advice—must give a large place to the history of science as the field on which the nations have always worked most easily together, used one another's results and helped one another, except in cases such as dye-stuffs or munitions of war, where war-like or commercial rivalry has disturbed the natural harmony of truth. In this study the citizen, who will in an international system be more than the citizen of one state, may find the means of strengthening those social feelings of the more intellectual kind which are weaker and limited in the merely national sphere. The sense of human dignity cannot be better served than by observing the growth through the ages of that quality in mankind as a whole which Aristotle taught us to regard as the differentia of man as a species. Our

sense of a debt to the past is most vivid in the case of that aspect of man's thought where congruence with the established results of earlier workers is most essential. And those earlier workers, even if hostile in the flesh, become, when dead, men *sans phrase*, all organs alike of one spirit whose nature is to struggle unceasingly for more strength, more clearness, and more comprehension.

Science here touches religion, as indeed any system of thought which involves the unity of mankind is bound to do. And the relation between the two suggests a contrast which has been often noticed before, but is of special interest to our argument and would lead us far if we had space to pursue it. The Middle Ages were noted for an internationalism in religion, which within its area—something like but something wider than the Roman Empire of the West—was the most intense and searching unity mankind has ever achieved or endured. The break came in the sixteenth century, and since that date it has been impossible, inconceivable even, to hold an oecumenical council for the whole West which would determine the right opinions to be held on any question of religion. But simultaneously with this dispersion on the religious side came a drawing together, a new internationalism, on the basis of science. The seventeenth century saw the establishment in the leading countries of the West of national societies and academies of science, which at once began to exchange visits, confer membership on one another, and advance by friendly rivalry. In the eighteenth century regular international co-operation began, and a multitude of permanent international associations followed, which are the most hopeful of all the prominent new organizations of the latest age. There are some hundreds of such, centred before the war chiefly in Brussels and The Hague; and their multiplicity is not due to difference of opinion but to variety of topic. They do not, as a Papal council would, declare the right opinion on a *filioque* clause either *ex cathedra* or through an official majority; but they welcome differences of view, supported by knowledge, on any vexed question within their range. They do not despair of agreement on any subject where evidence is available. But the agreement must be free, by conviction and not by force, by reason and not by authority. This is the unity of the future.

# FOUR ARMENIAN TRACTS ON THE STRUCTURE OF THE HUMAN BODY

By F. C. Conybeare



## I. Introduction

Armenian medicine was a closed subject until the appearance of Ernest Seidel's *Mechitar's Trost bei Fiebern* in 1908. That work, together with the texts here rendered, will enable the occidental reader to form some idea of the character and sources of mediaeval Armenian scientific ideas.

The British Museum Codex, Or. 6798 (Catalogue, no. 138),[1] is the source of our four treatises on the formation and Structure of the Human Body. It is a composite MS. formed of several distinct books and written by at least four different hands. The first section of this MS. contains our four treatises. This section consists of some 50 vellum leaves in double columns of 35 lines each. The writing is of the late cursive type called *notergir*, or notary's, small, but neat and clear. The titles of the sections or chapters are given in red. Folios 56–127 contain a second and separate book, written on paper in double columns of 32 lines, in a hand closely resembling the first. This second book gives our first two treatises over again in an identical text, with the same truncated colophon (see § 21 below). Either it was copied from the vellum book, or both were copied from a common source. As in the first book, so in this, the last tract is attributed to Gregory of Nyssa (§ 23 in our translation).

In addition to the two copies of our texts in the British Museum

[1] I would like here to correct an error in my catalogue of the Armenian MSS. of the British Museum. Misled by the numeral 3 affixed in the margin to the so-called treatise of Gregory of Nyssa on the folio of MS. 6798, I suggested that it was misplaced and should precede f. 4, in such a way that this treatise followed the Introduction and intervened between §§ 2 and 3. The folio was really in its right place, and the secondary text at f. 109b equally begins Gregory of Nyssa's tract after § 20 which describes the Seven Members, although § 21, on the Parts of the Body, is omitted.

volume, nine others are known to exist in a more or less complete state; of these, seven are in the Mechitarist Library at Vienna, one is at Munich, and one in the Bibliothèque nationale at Paris.[1] The four texts vary a good deal, but our version is practically identical with one of the Viennese (294). The great variety of readings indicates that the works had long been current and popular at the time when the variant MSS. were written.

The circumstance that the excerpt of Mechitar Heratzi (see § 1) occurs in at least two other MSS., and is followed by a passage which may be attributed to Asar (see next paragraph), suggests that the latter may himself have made the addition. As a physician Asar may well have had access to other works of Mechitar (who was a Cilician physician, *c.* 1150–1200) besides that on Fevers: that work the Mechitarists of Venice printed in Armenian in 1832,

[1] The Vienna MSS. 678 and 294 exactly agree with ours in texts and in the ordering of the contents. No. 658 of the same collection resembles it.

Another Vienna MS. no. 17 in Dashean's Catalogue, less allied in text to ours than no. 294, is a recent copy of an older MS. of unknown age; it gives the same contents in the same order.

The Vienna collection contains five other similar MSS. No. 436 is a late cursive. It begins with § 1 and ends with § 18, but Dashean's Catalogue gives no further information. No. 442 also contains it, and begins with § 1, but ends with § 17. The third MS., no. 466, begins with § 2, but what else it contains Father Dashean does not say. No. 254 is of the 17–18th century. The tract begins f. 7 r°, and contains §§ 1, 3, &c., but the codex is very defective through loss of leaves, and Dashean does not record its contents very fully. A fifth text is given in codex 540 of the same library, written in 1669. This contains the works of Galust, a physician of Amasia of that age. This text begins with § 1 and ends with § 20.

A Munich codex, no. 2, written in 1602, contains our text, beginning f. 71 r°, but I have only the barest details of its arrangement and contents, though Hunanean in his two volumes on ancient vulgar idioms of Armenia (Vienna, 1897), gives excerpts of—

A Paris MS., no. 108, ff. 29–45, contains the same text. Hunanean writes that he had examined ten MSS. of this work, and notes that two of them only, Vienna 294 and Paris 108, insert the excerpt from Mechithar Heratzi which I number §§ 3, 4. It ends with the words: 'So ends the description of the visual faculty by the will of God.' Our MS. continues without a break the rest of the discourse about the eye. Our MS. is thus a member of a close group consisting of three texts. Very slight differences divide them in respect of this excerpt; e. g. Vienna 294 and our MS. employ the word *Quawashs,* whereas Paris 108 substitutes *Vuslays,* and 294 and our MS. use the Middle Armenian plural *astarni,* whereas Paris 108 has the classical form *astarq.*

Hunanean cites § 1 according to the four codices, Vienna 294, Vienna 540 (Galust's codex), Munich 2 (written probably in 1602), and Vienna 17.

and a scholarly German version of it, with valuable commentary, was published in 1908 by Ernest Seidel. This treatise is much quoted in Asar's *Manual of Therapeutics* in the Mechitarist Library of Vienna (MS. 287), and is given twice over in a British Museum MS. (Codex f. 129 b and 41 a). It consists of 123 chapters, and cites many of the ancient Arabic and Armenian medical writers that are cited independently in the Great Tripartite manual of Medicine or Akhrapatin (i.e. *Γραφίδιον*) of Amirtovlath of Amasia, of which a magnificent vellum codex exists in the British Museum (Cat. Armen. MSS. 134, Or. 3712). Amirtovlath wrote *c.* 1466.

It is thus not impossible that § 1 of our tract is from the pen of this Asar, for in the preface to that writer's *Manual of Therapeutics* the comparison of the physician's art to that of the religious confessor (§ 1, paragraph 5) recurs in the same words. However, it is a commonplace often met with in Armenian medical treatises, so we must not attach too much importance to this. That one was copied from the other, or both from a common source, is, however, certain, since the language is the same. I translate herewith Asar's preface as it is found in our MS. (Brit. Mus., Or. 6798) and the Vienna MS. 287 :

'In the name of God the merciful and compassionate. A book of the healing art, as prescribed by wise philosophers and healing doctors for the understanding of man's nature and for ministration to the sick unto the uses of healing.

'For as by means of confession and true repentance they receive healing of soul, so likewise at the hands of healing doctors and with the aid of drugs they shall receive bodily health and be quit of their maladies. So now the humble in spirit, I, the servant of God's servants, the unlearned and much sinning Asar of Sebaste, have desired to collect the selection, and in brief to set forth according to our wants, a little out of much of the words of the philosophers, and to minister to sufferers, unto the uses of healing. And may the Creator vouchsafe health unto all according to his good and benevolent will, and to him be glory,' &c.

We may now devote a few lines to the discussion of the identity of the physician Abu Sayid, who is quoted in the first of our tractates (§ 1, paragraph 2). Two physicians of that name meet us in the history of Armenian medicine. The earlier was a contemporary of Gregory son of Vahram early in the eleventh century, and Amirtovlath cites his remedies more than once, e.g. in the following, which is given in Hunanean (ii. 415) :

'For liver disease due to fever we also copy out the remedy

used by Grigor son of Vahram much to his advantage at a time when he suffered in his liver through fever, and went to Mufarʌin (i.e. Nfkert) in the year 1037. And it was a prescription of Busayid and did him good. And his symptoms were these: pain in the back and right arm and heaviness of the hand, and internal stabbing pain in the back where the ribs fall away. When he lay down on his right side he felt acute pain and grew feverish, and wine and anything he ate hot gave him constipation (*or* ? aggravated it). So when this malady came on he went to Mufarʌin; as it was in winter time, the doctor gave him no medicine, saying: At this time of the year drugs will do you no good, for the man is frozen like the earth, and drugs are useless. But he gave him cool drinks, such as pomegranate liquor, &c., and let him eat what he liked. Then when spring came he prescribed him this treatment for forty days,' &c.

The Armenian prince evidently suffered from neuritis. Here is another mention of Abu Sayid from the same source:

'Another remedy which we have copied from Abu Sayid's manual of medicine, which Gregory son of Vahram used when his liver pained him and he went to Mufarʌin, as he did every spring, and derived great profit therefrom for several reasons: Take damask plums and twelve jujubes,' &c.

This Abu Sayid of 1037 was probably a Syrian or an Arab, but some of his writings were clearly preserved among the Armenians as late as the second half of the fifteenth century, either in the original or in Armenian translations.

Rather more than one hundred years later we have a notice of another Abu Sayid, a physician and savant, who was a friend and correspondent in turn of Nerses Shnorhali the Graceful, patriarch of Sis in Cilicia, who died A. D. 1173, and of Nerses of Lambron, bishop of Tarsus, who died 1198. Shnorhali, in his commentary on St. Matthew, states that he consulted this Abu Sayid about the reconciliation of the rival pedigrees of Jesus in the first and third gospels. He calls him a physician and savant, and wanted to know what solution was provided of the difficulty in Abu Sayid's Church. This proves that Abu Sayid was not an Armenian but a Syrian Christian. It was also at his request that Nerses of Lambron composed his tract on the *Names of City Builders*, published in the *Ztschr. f. Arm. Philologie*, 1903, I, p. 206. As Nerses wrote in Armenian, we infer that his friend Abu Sayid could at least read that tongue. He probably wrote in Syriac or Arabic.

It is impossible to say for certain to which of these personages the reference in § 1, the Prologue, refers. The circumstance that the tract is in the Middle Armenian idiom of Cilicia proves nothing, for if it was originally written in Syriac or Arabic, an Armenian might translate it as well later as sooner. There is, however, some evidence for Nerses of Lambron being the translator, in which case it is likely to be the work of the Abu Sayid who was his contemporary. This evidence consists of three notices to the effect that this Nerses composed such a treatise. The first is found in a short but anonymous life of him cited by Alishan, the modern historian of Armenia, in his volume of *Sisuan*, p. 91. The second is in a colophon printed, apparently from the MS., of Nerses's meditation and prayers in connexion with the *Dormitio Iohannis*. The third is a notice, printed in an edition of sundry works of Nerses printed at Cpl. in 1736, to the effect that he wrote the book on the *Formation of Man*. On the strength of these notices Hunanean inclines to believe that Nerses of Lambron was the translator. He was certainly familiar with Syriac, for we have Armenian versions of Syriac originals from his pen. Hunanean confesses himself unable definitely to fix the date of the tract from the language in which it is composed, but finds no difficulty, as we have seen, in attributing it to Nerses of Lambron, who died in 1198. I find myself a great affinity between its idiom and that of Mechitar Heratzi, the author of the work on Fevers. It would be out of place here to go into details, and I will mention only two striking facts. Both in it and in Mechitar we find *ukhtavoruthiun* for *akhtavoruthiun*, a sign, though one rarely encountered, of the phonetic decay of the vowel *a* in that age. Again, instead of writing *erkouorek* for ' testicles ', both writers employ the form *ekavorek*. This is a rare form, so rare that Dr. Seidel, excellent scholar as he is, has not understood it. I confess that I can in general see no distinction between the Armenian style and idiom of Mechitar and that of the author, whoever he was, of our tract. It is possible that Mechitar, who was a friend of Nerses Shnorhali, and wrote his work on Fevers in the year 1184 (when Nerses was Patriarch of Sis), may himself have executed the translation of Abu Sayid at the wish of Nerses. It was a common thing for learned men to undertake such tasks at the behest of a prelate ; and that may be the reason of Nerses of Lambron's name being attached to it.

As regards our fourth treatise, it is needless to say that Gregory of Nyssa, whose name is attached to it, had nothing to do with the work, and that the tract of which I have here (§ 23) translated the first few pages is falsely attributed to him. It awaits more complete treatment than I have been able to give it. It was the connexion in literary tradition of this Father of the Church with Nemesios which gave rise to such an extravagance. The work of the latter exists in Armenian, but has nothing in common with the work ascribed here to Gregory of Nyssa.

We have, then, in these four treatises a monument of the medical learning of the Armenians not later than the twelfth century. It would need a wider acquaintance with the many MSS. of these works than I have had the opportunity of making, to decide whether and how far the texts have been amplified by medical editors and scribes like Asar of Sebaste. We must not, for example, without further inquiry, attribute to the original form of the treatise the ascription to some planet or other of each organ of the body. These ascriptions in our work invariably come at the end of the section devoted to the particular organ, and may easily therefore be a later accretion from the pen of Asar, who in the colophon of § 21 admits that he in some way completed Abu Sayid's work, and who no doubt incorporated in it §§ 3, 4.

Like the later medical schools of Europe, the Armenian was dominated by Arabic learning. Most of the technical terms used are Arabic, much disguised in their Armenian dress. Equally so are the names of Greek medical writers that often came first through Syriac, and from Syriac through Arabic. Bagarat, for example, a common and distinguished name in Armenia and Georgia, disguises Hippocrates. In other Armenian medical treatises we have Archigenes disguised as Ardjidjanes or Ardjiasus, Paul (of Egina) as Flaus, Oribasius as Arpisaus, Rufus as Upufaus, Diogenes as Deudjanis, and so on.

## II. Contents of British Museum Text

[Press-mark Or. 6798 ; Cat., no. 138, fo. 2–11]

The text that we here print contains four separate works, of somewhat different style and motive :

I. § 1–§ 18 is a complete and systematic treatise on the structure and functions of the organs of the body. It is perhaps the work of the physician Abu Sayid, who lived in the

twelfth century, but his work has been amplified by one or more medical scribes such as Asar.

The British Museum MS. has a colophon at the end of § 18 attesting that ‘ This Book was written by Halathzaden ’, and we are asked to remember Astuadsatur the Elder, ‘ our father ’.

Whether ‘ this book ’ refers only to the treatise which precedes is not clear. It is, however, an indication that §§ 19 and 20 formed once a work separate from the treatise.

II. § 19–§ 21 is a separate and more theoretic work, which deals chiefly with the numbers of the various organs and with their relation to the mental and spiritual qualities and with the causes of disease. The colophon expressly states that it is imperfect.

In the British Museum Codex Or. 6798 this colophon, § 21, is truncated. But in the Codices of the Mechitarists' Library at Vienna, 678 and 294, which otherwise presents a text identical with ours and which was copied from 678 in A. D. 1625, it runs thus :

‘ *Now, Brethren, our original was very imperfect and faulty*, but, by the help of God, Asar of Sebaste (Sivas), the scribe and true disciple of the book, having with excessive erudition given his leisure to foreign works, with much labour was *barely able to bring it to so much accuracy as this.* But *you that are aided by it, bear in mind the sinful* much toiling Asar the Scribe and myself, the sinful penman who has soiled the pages of the paper and am also called the penman. Mark, O beloved among sages, to accept from me the word of the Apostle Paul, that there is won of your goodwill the grace of the Lord ’ . . .

It is highly improbable that the author of this longer notice would have gone out of his way to incorporate in it the phrases italicized from the shorter notice. The MS. which thus enables us to restore the colophon was written in 1625 at Ispahan by one Paul the Monk.

III. § 22 is a short note on the relationship of the various organs to each other. It was, as its colophon tells us, transcribed by, if not the work of, one Halathidy.

IV. § 23 is a spurious work attributed to Gregory of Nyssa (*c.* 331–*c.* 396) on the formation of the foetus in the mother's womb. Only the first part is here rendered by way of giving an idea of its contents and character and of identifying it.

The tractates here translated consist of the following sections, the numbers affixed being my own and not those of the MSS. :

I. 1. Introduction.
2. Concerning the Head and Brain.
3. Concerning the Eyes.
4. Concerning the Muscles of the Eye.
5. Concerning Vision (in the text no title is given).
6. On the Ear.
7. On the Nose.
8. On the Mouth.
9. On the Heart.
10. On the Lungs.
11. On the Liver.
12. On the Spleen.
13. On the Kidneys.
14. On the Gall.
15. On the Bladder.
16. On the Testicles.
17. On the Stomach.
18. On the Guts.

II. 19. On Sinews (*or* Nerves), Ducts (*or* Veins), and Blood in General. Begins : 'The all wise God formed the joints of man . . .'
20. On the Seven Members (*or* Organs) whereby man hath Life. The first is the Brain.
21. Colophon.

III. 22. The Parts of the Body.

IV. 23. A work attributed to Gregory of Nyssa, beginning : 'Man is said to be of four constituents.'

I have to thank Dr. Singer for supplying me with photographs of the British Museum MS. as well as for many suggestions ; and my gratitude is especially due to Father P. N. Akinian of the Mechitarist Convent of Vienna for the care with which he has revised my translation, correcting it in numerous passages and furnishing the right meanings of many obscure terms.

## III. Translation of the Four Tracts

### TRACT I

1. *Concerning the formation of man and the creation of all the members* (or *organs*) *of man, by the will of God.*

Of truly able select philosophers and healing doctors, Hellenes and Greeks, for the understanding of the nature of man's body, mouldings and members, bones and articulations, ducts (*or* veins) and sinews. How they were created and what are their respective natures or functions, whereby they supply with moisture all the members [1] of the body.

But also the exciting causes (lit. movements) of diseases and the remedial aiding of the same, and the operation, as understood by the great physicians Galen, Aristotle, and Bagarat (Hippocrates), by whom [the remedies] were disseminated among Greeks and Assyrians and Persians and Indians, among Hellenes and Arabs, and were disseminated unto all the corners of the world by the Giver of grace from above, and there were able men of all races. And at their behest and by their words many investigated and made themselves wise and able. Among whom was also one called Abu Sayid, who took from the books of the chief physicians sincerely and concisely, and bestowed on us this treatise, correct and succinct, unto the praise and glory of God, who fashioned creation and equipped all with utilities as he willed and made all, and what he commands comes to be.

And who is able to search out the deep things of God? for whatsoever God made is exceeding good.

God made water, earth, sea, and dry land, beings of fire and beings of clay, beings spiritual and those that breathe, animals and birds, plants and vegetables, and all else. God made the body of Adam, and vouchsafed to him rational spirit and charged him to love God and keep his commandments. And the love and science of man, to discover this was the art of healing, by means of healing doctrine and co-operation of drugs to minister to the suffering unto the uses of health.

For by means of the confession of sins and acceptance of repentance a man shall receive healing of soul, and at the hands of physicians and with the aid of drugs he shall receive bodily health by the will of God, as saith the prophet: He that hath not bodily health, cannot serve God in spirit.

The wise [2] Galen says that God created man like a city, having twelve gates by which drugs and foodstuffs enter, while superfluities go out, whereby the system (*or* person) is constantly aided.

And of these twelve gates, of which we spake, two are eyes, two ears, two nostrils, one the mouth, two the breasts, one the navel, and two exits, one for discharging water and the other the posterior.

But there are two great channels on the two sides of the haunches (buttocks, *lumbus*, *ilium*, or *coxa*). And on each side of the haunch 180 channels open, which makes 360 ducts in movement, from all of which the members derive material and are strengthened.

Also as there are four winds which blow over the world, from East, West, South, and North, and as the year is divided into four seasons, Spring,

[1] *Member* is used both of internal organs, heart, liver, &c., and of external limbs.

[2] Perhaps the treatise of Abu Sayid begins here rather than with § 2. In any case § 1 up to this point must be a composition of Asar's, the editor or redactor of Abu Sayid's work as we have it here.

Summer, Autumn, and Winter, so the life of man is divided into four portions. For in childhood and the first age, man's nature is hot and moist, because it is dominated by blood and eastern air and follows the spring season, for the nature of spring is hot and moist. But in the second age, while youth lasts, nature is hot and dry, being dominated by bile and southern air, and follows the summer season, for the nature of summer is hot and dry.

And the third portion of life is in nature cool and dry, being dominated by bile (savta, χυλός) and western air, and it follows the autumn season, for the nature of autumn is cool and dry. And when man enters the fourth portion, it is old age, and his nature is cold and moist, for it is dominated by phlegm (palλam, *pituita*) and north wind, and follows the winter season, for its nature too is cold and moist.

The wise say that when God created Adam it was springtime, and night and day each consisted of twelve hours, and the sun was in Aries in the first degree. Therefore when spring comes, everything turns green and sprouts up out of the ground; and all animated beings are stirred, and humours (lit. minglings) of body are subtilized, and blood and bile ferment and are rarefied. Wherefore all physicians have bidden in spring days to bleed and imbibe purgatives, for all men's humours in this manner are made to ferment and in these days grow soft and rarefied. But there are formed in the person of man four kinds of liquid, salt and bitter, sweet and ill-smelling. Salt liquid is of the eyes, for were it not salt it would melt the fat of the eyes. And bitter liquid is of the ears, for were it not bitter, flies and creeping things would enter the ears, and a maggot would be there and do harm. And sweet liquid is of the mouth, which receives the savour of things eaten. And ill-smelling liquid is in the loins, whence comes seed, and offspring is generated therefrom.

So far so good.

2. *Concerning the structure of the Head and the Brain.*

The wise Galen says that God made the human head and set within it the brain, and of all wisdom and faculty of movement did he set the seat therein. God made the human head in three portions. The first he made the place of sensation and of light's filaments of the eyes. The second portion he made a vessel of the consciousness and of the intelligence (antidjeli, *djeldsch*). And the third portion he made the place of guarding, that whatever it sees and understands, therein it may guard and study it.

And God devised the head of seven layers (*tapaλa*) and seven membranes, in every layer one membrane. For all these are a protection of the brain that there may reach it suddenly no whit of mischief (? *zenuthiun*) nor any pain. The first layer is the hair; the second the skin; the third the flesh; the fourth the bone; the fifth is another skin enclosed within the bone; the sixth is a skin over the brain and interiorly another; the seventh is the brain. And God instituted all this protection of the brain that chill or heat should not be able to penetrate to it, and do harm (? *zên*) to it; for the brain is master of the house and in command of the heart. And the heart is sovereign of the whole person, and than the heart or brain there is no more excellent (*aλêk*) member in the body. For health and life reside within the heart, but intelligence (*or* consciousness) and initiative in the brain.

And the hair of man was made by God and devised out of consumed (or burned) blood, and in proportion as the consumed blood increases, the hair takes increase (? *λalapa*) and waxes strong and long; and more and more this arises from the blood which is consumed, for while the man is alive and healthy it is made from pure blood. But when fleshiness (? *mis*) increases

and phlegm (=*pituita*) in him, the hair does not sprout, and the man whose hair grows thin, his nature becomes phlegmatic; but if a man's head is grown bald, that is due to red bile. But man hath grace and shame in the brain, and when a blow (*or* pain) affects the brain, it causes a lack of two things in the man, and the man becomes unconscious (lit. silly) and insensible, so that he cannot know and understand what is good and what bad. And if the blow falls on the middle of the brain, which is the seat of domination and consciousness, or if it be excessive and concentrated, the mind fails and he swoons and can comprehend nothing.

But if the injury befalls the posterior cavity which is the retentive (? ἑκτικός), forgetfulness comes over him, so that he knows no more, either past or future. Medical men have said that they knew cases where men were so injured, with the result that they forgot the names of their own fathers and mothers who begat and bore them. And there were some who, when they were yawning, even forgot to shut their mouths, so oblivious did they become. And this affection visits men in grey-haired age more severely than at any other time, and interiorly is due to phlegm (*pituita*).

And there is, furthermore, a path leading from the brain to the heart, so that the brain incessantly has the heart in view; and when this path is open, a man is subject to the disease called swooning (*saqthay* = syncope); and it often happens that when this affection prevails, a man is bereft of consciousness and intelligence, and the colour of his face goes. And not a few physicians through ignorance of this affection imagine the patient to be dead and hand him over for burial, for one who is suffering from it is as it were dead and the colour leaves his face. For when the passage to the brain from the heart is blocked, the heart is unable to absorb water any more from the brain, and the life of the heart and its warmth are unable to reach the brain, with the result that the latter is congealed; and then this affection occurs and manifests itself in the man. For if life and illumination and substance were not in (*or* from) the heart, the brain would quickly be congealed and the man die.

But if there were not coolness and moisture in the brain to counteract the heart's heat, this heat would rapidly consume the entire person. And God made the brain cool and damp, and set the moon over it as its controller [1] (*or* arbiter), but made the heart hot and dry, and set the sun over it as its controller. And these two by their nature give strength and support to one another, and substantiate each other, and they fare well with God's help.

3. *Concerning the structure and formation of the Eyes.*

The great Mechithar has said that every physician who wishes to tend the eyes must study the structure of the eyes, treatment of which is [given] by philosophy; and in these recipes (*vuslays*) [2] the eye was not described, not even in the *alrapatin* (γραφίδια, *pharmakopoiia* or *therapeutica*). So then, I, Mechithar, was minded to describe the formation of the eye in brief, and relate how many tunics (*tapala*) there are which are linings (*astarni*) of the eyes, or how many *rutupat* which are humours of the eyes, or how many tendons *azaltunae* (Arab. *zala*) which are muscles of the eyes. And I describe the visual (*or* contemplative) spirit, where it belongs and how it proceeds and progresses along conjoined (*or* equal) fibres which they name the *Lusénion* (i. e. retina): it is necessary to know this, because ancient

[1] *tanuter*, an astronological term, lit. house-lord.

[2] *Nuslay* or *Nuskhay* in Paris MS. 108. Our MS. and Vienna 294 read *Qunnash*: ? an Arabic word.

philosophers have somewhat contradicted each other concerning the membranes of the eyes; for some say there are six; others five, others four, others three, and some two tunics. But I hold with Galen, for Galen and his following said there are seven [of] the eyes, and three humours and nine muscles, and I mention all, one by one, by the aid of God.

We now describe the tunics of the eyes.

Now the first tunic that is interior attached to the bone, which the Tadjiks (Arabs) call the *zulpie*, which is to be translated the hard body (i. e. sclerotic); it is more sinewy and firm and hard than the other membranes, which is why they so named it, and it attaches to the bone which separates the bony ruggedness (?) from the eyes, and protects the eyes and prevents mischief (*zahokn*, accus. of *ahok*) from getting into the eyes. For the first meninx (*mizλ*) is interior, and of the cranium there are two meninges, the one (attached to) the bone and the other attached to the brain. Now the one attached to the bone, hard is its body and sinewy, and it possesses many veins (*or* ducts) from the artery, and its use is to keep off the bone's ruggedness and weight from the brain, and prevent injury (*tchahok* ?) to the brain.

And the second meninx which attaches to the brain is more delicate and soft and pure than the first, so that it may not weigh on the brain, and its body likewise is subtle [1] of veins, and of the artery. And this membrane which is named sclerotic, is engendered of (*or* ? engenders) this meninx which attaches to the cranium.

And the second tunic is that which is named *shmima*, i.e. placenta; this they call *sekin* (choroide), and they call *shmima* the skin which covers a child in its mother's womb, and it is born with this membrane; by this metaphor they have illustrated it, and have called it by this name (viz. *mater*). And it belongs to this meninx which lies upon the brain, as we mentioned before.

The third tunic which they have named *lapaqia*, which is to be translated *ark* (i. e. retina), for it has the semblance of an ark, wherefore it was so called, and the thing owes its origin to the placental (? *shmima*) membrane.

But after this come three humours. There is a humour which they have named glass (*zudjadji*), which is to be translated *apikeni* [2] (i. e. vitreous), because it has the semblance of white glaze. Wherefore they have so named it.

Now this is followed by a humour which they have named *djaliti*, which is to be translated *sarneni* (or crystalline), pure and resplendent and circular. and you must know that the crystalline is a conspicuous (lit. glorious) and precious appurtenance of the eyes. . . . For through it arises the visual (faculty) and perception of colours and forms, and its roundness is to the end that it may not incur mischief (*tchahok*) and adverse shocks, impinging on it. For the reason that the arteries in those places remain at peace from the roundness, and the crystalline (ternic) is in the middle of the eyes like a ball. held in the midst, or like a (central) point in a circle, and it is surrounded and protected by all the membranes, and humours subserve the precious (thing), in order to ward off mischief (*tchahok*) and secure its welfare continually

And after this is the fourth tunic which they name *yanqaputhia*, which is translated *sardosteni* (arachnoid), because it resembles a spider's web, and is subtle and limpid and pure, wherefore it is so called. And it lies between the crystalline and the white of egg in the middle, that mischief (*tchahok*) may not from its humours happen to the crystalline.

[1] Perhaps the sense is ' delicately veined and supplied with arteries '.

[2] In Paris 108 *apakini*.

And besides this there is an humour which they name subtile white of egg (i.e. aqueous humour), because it resembles egg in whiteness, and therefore they have so named it.

Fifth is the tunic which they call *yanapia*, to be translated *khaλoλeni* (vine, uva), because it resembles that fruit of the vine, wherefore it is so named.

And the sixth membrane is named *kharnopia*, to be translated *eldschereni* ( = *cornea*), which is why they have so named it. In itself it is exteriorly limpid and resplendent and smooth as if hard skinned, and this is the reason why when you open the eye you see the image.

But Gelianos (? Galenus) said these three tunics were one (or the first) meninx, and appealed in witness to the fact that when *karha* arises, which is a tumour (*or* ? blister, pimple, &c), and if it issues upwards into the *aponeurosis* (*mizλ* or ? = *amnios*), it is quickly inflamed (lit. boiled) and at once opens and the *spin* (cicatrix) due to it is soft and there is not a white facula, and a remedy is quickly ascertained.

But if the tumour (?) is in the second *mizλ* (*aponeurosis* or membrane), it inflames (*efi*) and opens late, and the *spin* (? cicatrix) which is due thereto (?) is thicker and white in colour, and a remedy is quickly ascertained.

But if the tumour penetrates the third *mizλ* (? membrane), it is late to inflame and the *spin* (? cicatrix), which comes in it, is denser and firm and the colour, a white facula (? *dschah*), and no remedy whatever is known.

And if the tumour is large, so that it bursts itself, or the matter (*Khltv* = ὕλη, χυλός) be acrid (*sur*, *or* sharp), so that it opens, and the uvea appears exposed, if it appears small, they call it a musca (fly in the head).

If it appear large, they call it a *bepp* [1] in the head, and name it *karhay* (i.e. wound), and also they call it *bath* and *khaλuart* (abscess).

And there is a seventh tunic called *multhahimay*, to be translated *Koshrads* ( = *conjunctiva*), and it is inside like a mantle (or shelter) to protect exteriorly, and it spreads out upon the membranes, wherefore they gave it this name, for it is a cartilage (*khrdjtam*). Wherefore they make a wide perforation in it and let pass the water, named tube inside like a *kamsh* (i.e. *gamysh* = reed-tube), and open the eyes by God's will.

4. *Concerning knowledge of the Muscles of the Eye and their function.*

And be it known that the eye has four chief muscles : one, on the upper side (*dih*), which draws up the eye, towards the eyebrow, and one on the lower side, which draws the eye down, towards the nose and cheek, and one on the side of the source (or fountain), which pulls the eyes towards the eyebrow, and another on the side of the ear ; and there are four to the four sides, strong (? *brnen*), and if to any one of them humour penetrates, and they relax, the eye droops and drops and goggles, for it lengthens and is drawn back obliquely. But if one of them is affected by dryness, it is drawn back to the other in the same way, and that distorts the eyes, for whatever dries up the eyes drags them up and makes a squint, and this is the cause of squinting.

And there are two other muscles which they call *thevq* (wings). It is they that move the eye in a circular direction, up and down and from side to side, as a man desires.

And there are three other muscles destined to control the tube of the nerve fibre, at the end where the pupil is, so as to concentrate and keep the light in it. And if the mixture of these muscles be moistened and softened,

[1] *Bepr*, Persian for leopard.

or if they be lacerated by any outside shock, the light is poured out and dispersed all over the eyeball (lit. fruit), which they name *inthishar*, and which they translate *vathats*, i.e. outflow.

But if their composition dries up, and they contract, as for example a thong (or rope), falling into water, relaxes and is stretched; and if it falls into fire conversely contracts and shortens (lit. comes together), so now the muscles, when they are wet, become *inthishar*, and, when they dry up, draw together the tube of the sinew at its origin, and the pupil is compressed. And the pupil appears the eye of a needle. And this is what they name compression or contraction.

And there are three sinews (muscle or nerves) which control the upper lids of the eyes, and their function is to draw the two lids down. And one draws up the lid and opens the eye, but the lower lid has no sinews, and for that reason is not moved, and if it moves does so unnerved (lit. by non-muscle or non-sinew).

And these are the nine muscles in the eye we wrote of, three supplying the upper lid.

Now I describe the tube of the muscle (*or* sinew *or* nerve) and say whence it is generated, and how it goes to the eye, and its use or function. And we must remark that the head has three cavities: one on the hinder side in the occiput, one on the front side in the forehead, and one in the middle. And the hinder cavity contains the faculty of memory,[1] the middle one the understanding, which is the brain, and the front one the senses. And from the front cavity springs an united (*or* conjoint) nerve, which they name the first pair or conjoint, and at middle distance (*or* in the middle of its position) it has as it were a tube, and proceeds straight along the right side to the right eye, and leftwise to the left eye. And when they reach the inner part of the bone of the forehead, they confront each other and mingle and become one, and separate afresh at the same spot, and pass along the right side to the left eye, and leftwise to the right. And the reason why God so arranged is that in case one eye be blinded, the light collects in the other and is gathered in it adventitiously. And we have evidence of this in the fact that (he that) wishes to see things clearly, that is, things dim or afar, covers one eye for the light to collect in the other eye. And this Nature has taught us and made clear, and this is the tube of the nerve, along which visual faculty passes and reaches the eye.

Now I have to mention the visual soul, whence it arises and how the visual (faculty *or* object) comes to be through it,[2] when the stomach dissolves and exhausts (or presses) the food, and sends it on to the liver, and the liver receives and concocts it, until it is converted into blood; and in this concoction an exhalation rises as from all things in process of being cooked (*or* boiled), and this air, so far as it is in the liver, they name a spirit of Nature. Now what of it is limpid and pure (decent *or* normal *or* temperate) goes to the heart, and there is named vital spirit, and what is pure in it ascends by the ducts to the brain and enters the firm meninx, which is within the cranium, and there circulates along all the ducts and is further concocted and purified. And then it enters the second meninx, which is above the brain. In the same way it circulates these along all the ducts and is cooked and purified.

But nature which stood in need of this warm vapour knew well how to refine it, wherefore she made the road a long one and the passages narrow.

[1] Read *isholuthiun* for *ishol.*

[2] The transition is so abrupt that words may have dropped out.

And so it enters the front cavity of the head, and is there named the perceptive spirit, and when it is there sufficiently refined it enters first the conjoint nerve (*or* muscle), which contains it in its midst like a tube, and so it passes to the eyes, where it is named the visual spirit and accomplishes vision, through the moisture of the crystalline, and through the mediation of the tunics. So ends the description of the visual faculty by the will of God.

5. And you must know that the benevolent God set surely the faculty of vision in the middle of seven tunics, and made them a protection that no ill may reach it; by way of convenience also in order that the brain's moisture may do it no harm.

And as for the water of the eyes, he provided it that the heart's warmth and wind might not do harm to the eyes. And on the outside of the eyes God has provided two sets of hair, one on the lower lids and one on the upper lids.[1] The one he made for the sight and the other to carry away moisture from the eyes; for if there were not eyelashes, the water of the eyes would continually be leaking out. For see you not that when the lashes are kept away from the eyes, tears come regularly. If there were not upper lashes a man could not see anything from afar. The eyelashes enable him to see what is near, and the upper lids what is afar. And the latter it is that keep off the sun sufficiently for it to do no harm to the eyes. And a man whose eyelids are removed is like a channel into which the water enters, while there lack trees and grass along the banks; the channel is ruined by the soil filling it up. For the trees and grass along the banks kept it from being damaged. So with a man's eyes; when the hair is cut away from the lids he is liable to many maladies of the eye. God made man's eyes of light, and the sun takes light from light. When a child is separated from its mother and in that hour the moon is in star chamber of Zohal (Saturn), the child becomes *atchika* (blind or ophthalmic), for when the moon is in a foul star chamber, the seed of disease enters the eye.

6. *On the formation of the Ears.*

God fashioned the ears for hearing, and the audition of man was arranged by him within the brain. And he appointed two ducts leading from the brain outwards to the ears. And God made the ears like a strain of music (*or* like a goblet) *speurô* (? spiral),[2] which when you strike it, gives a sound like a flute; and what is heard, it transmits to the brain which takes it in, and makes of it what is convenient.

And many men are deaf from their mothers' wombs, and that is owing to the fact that these two channels are blocked up. But those who being grown up go deaf, the cause is this: that from a superfluity of bilious matters a vapour ascends to the brain, and from the brain issues into the ear to the two channels and fills them up, and stops the hearing. For the passage by which we hear is then blocked. And often enough the cause resides in the phlegm (=*pituita*), for an excess of this affects these two channels, and the ears, and stops them up, and denies you hearing. Scabies (ψώρα), however, does not create phlegm. But of black bile and of red bile and of blood a superfluity creates severe scabies, and they interfere with and prevent hearing.

God made the ear like a cellar (?) and built its entrance with a twist, so

[1] The word *Irander* 'upper lids' usually means 'eyebrows', but to so render the passage would make nonsense of it.

[2] The Vienna MSS. read *spetrô*, which recalls *spectrum*, but that gives no good sense in the context.

that foul air might not enter, nor an injurious sound easily enter the ear, and reach the brain and do harm. And (God) put in a naturally bitter liquid, and did so to check any harmful insect that might like to enter the ear, for it encounters the bitter liquid and is prevented from entering ; for because of its smell and taste they do not venture within, but flee and go back.

And God made more precious than the whole person the eye and ear ; but some physicians have said that the ear is more excellent than the eye, because, when it is dark and night-time, the eye cannot see anything, whereas the ear, even if it be night or day, hears everything, and, what is more, hears better by night than by day. And some physicians have rated the eyes higher than the ears for the reason that, if you do not hear a thing with your ears, you cannot tell another about it ; whereas the eye sees everything, and describes it to another person. And by way of example they adduce the thunder and lightning, but the eye sees the lightning and only afterwards the ear hears the thunder.

But God made the ear because of the brain, so that whatever is heard, is sent on to the brain, as the gentleman's doorkeeper does, who does not allow everyone, especially the unsuitable, to enter his master's chamber (*or* court), until he receives an order to do so from him, and then lets him enter. Just so the ear, which does not allow the unsuitable to come in.

7. *Concerning the formation of the Nostril.*

God formed the nostrils in order to perceive all good or bad odours, and to expel and disperse all the brain's superfluities. And he fashioned the nose like a conduit to protect the brain. It gathers and keeps therefrom any superfluities. All this issues out by the nostrils.

And from the nostril a passage was devised by God to the windpipe, which when the mouth is shut lets the breath issue forth ; for were it not so, the mouth would have to remain open, and there would be a great risk of creeping things entering the mouth and doing harm, so that many would die of it.

And God made the human mouth like a box of which the padlock (*kuplaq*) is a rampart gate.[1] And the nose was created downwards, firm and solid out of cartilage and free from the food that is being chewed, so that when the mouth is full, the breath can issue forth through it. And the fumes of bilious matters issue forth by the nostrils, and the breathing is not hampered nor the man strangled by them. And these fumes and superfluities in this way cannot enter the brain and harm it. For there is then an ample passage kept open, and, as long as it is open, this injury cannot befall a man. And the discharge of the nostril was made salt in flavour so that foul irritants (? *havaj*)[2] may not enter, and the brain remain uninjured. And if a thousand times the nostrils were open still creeping things would not enter, for its liquid is salt, and its presiding star is Lucifer. But God made the countenance (*eres*) of man fiery and temperate, and lo, it is clear that winter does not freeze it with cold nor summer torment it with heat, nor is it burned up by the sun, by the command of God.

8. *Concerning the formation of the Mouth.*

God fashioned the mouth for eating, and made its liquid sweet, in order to receive the savour of all things ; and he fashioned the tongue in it in order by its continual movement to keep the mouth moist, and we receive the flavour on the roof of the mouth (i. e. through the palate) with the tip of the tongue ; for have you not remarked that when the tongue is cut off, the

[1] Probably the reference is to a battlement projecting above the main portal of a fortress and perforated beneath with apertures.

[2] The Vienna MSS. read *haut*, Arabic for *air*.

savour of things is lost and no longer perceived ? And the liquid which is collected in the mouth is generated in the liver ; and when it ascends from the liver, it supplies blood as far as the windpipe, and then by God's command it becomes white and turns into water. But if red bile is left in the liver, it causes the savour of the liver to be bitter ; and the water which is in the mouth becomes bitter. But if black bile is left in the liver, the savour turns sour, and the mouth becomes sour. But if phlegm is left in the liver, the savour of the mouth becomes salt ; but if there be only pure blood in the liver, and the liver be healthy, then the savour of the mouth is sweet and *mah thatil* (?).

And God made in the mouth a passage to carry to the whole person flavours. But the presiding star of the mouth is Jupiter, while as that of the tongue was appointed Mercury, and that of the teeth is the same Mercury. For have you not remarked that when a man is in Mercury, he is soft and twistful in his speech, and cannot talk trenchantly, and a man who lacks teeth cannot fully enunciate his words ?

9. *Concerning the formation of the Heart.*

God fashioned the heart as sovereign of the entire person, and made its nature hot and dry, placing the entire heat of the person therein, and constituting it the abode of warmth, so that the whole person is heated therefrom. The heart itself is perpetually in movement and has no rest, like the heavens which know no repose at all. So too the heart. And God made the heart like a tree which is broad at one end and narrow at the other. And the entire heart consists of two chambers.[1] There is one chamber on the right which contains living blood, and one on the left which contains air. But from the heart proceed four ducts, of which each divides into 32, so that in all there are 128 ducts, which the Arab calls its arteries. And together with these ducts the heart is in continual movement ; and they are evidence of life and death. For when skilled physicians lay their fingers on the duct which is called *madjas* (radial artery ?) they diagnose from every side the ailments connected with death or life, and apply the requisite drugs. And of these four ducts we mentioned, two are distributed to the upper parts of the body, and two to the lower, and they give force to the entire body, so that it is strengthened by them and keeps healthy, and as its presiding star was appointed the Sun.

10. *Concerning the formation of the Lungs.*

God fashioned the lungs and constituted their nature cool and moist, and made it the chamber of phlegm which is *malas*. And God made the lung as the breath of life of the whole body, and placed it between the heart and stomach to be continually the refrigerator (?) of the heart, and allay (?) its ardour. And it also allays the heat of the liver, and prevents the heat and ardour of either of them from consuming the body. And God has constructed the lungs like an oven, that the air in it may constantly circulate within. If it were not so, a man could not at all keep his bowels (*or* middle) cool. And God made the lungs an air-chamber, to distribute the wind over the body, and wherever it is wanted it is made to go as required. And its presiding star he made to be the moon.

11. *Concerning the formation of the Liver.*

God made the liver to preside like a good or evil star over the whole body, and made it the fountain (*or* source) of blood. And he made its nature hot and moist, and constituted it the furnace of the stomach, so that any

[1] Or, ? ' And the circle of the heart is to be set firm upon two chambers '.

food which enters the stomach is cooked by the liver by its heat. And when the food is cooked, the liver draws its virtue (*or* strength) into itself, and in accordance with its nature converts it into blood, and distributes it over all the members, and feeds and strengthens the body.

At first it sends to the heart the simple (*or* clear) exhalations (*or* vapours) which issue from itself, and having reached the heart they are afresh distilled or rarefied and so rise to the brain. But the bitterness which like sulphur (?) escapes upwards, it sends to the gall to become yellow bile (*safra*); and the other crude (uncooked or undigested) blood it contains as a moisture and heavy liquid, it gives to the lung, to become phlegm (*pituita*); and the blood, which possesses fat and is crude and mixed with water, it gives to the kidneys, to be cooked and become semen; but whatever is not accounted for goes to the *falabusht* or bladder, which discharges it without. But the rest of the burned blood is collected at the buttocks (*or* on the bottom) as sediment (τρύξ), and the *aqpuq* (?) gives it to the spleen to become bile.

And in this way the liver feeds all the members of the body.

But if it should happen that some indisposition affect the liver in the way of heat or cold, moisture or dryness, or if it be due to obstruction caused by constipation, the liver is oppressed and cannot get to their destinations its contributions to the various members, and these superfluities are left in the liver and cause it injury; and enfeeble not it alone, but all the members, and the body is consumed and peaked and is in danger.

But if the liver is oppressed by cold, and cannot warm the stomach, so as to cook (*or* digest) the food, the latter is left undigested (uncooked) and becomes bile, and thin water; and this water the liver cannot take up (? absorb, lit. bear weight of), but it disperses it through the body along the fibres and joints (*or* seams), and it causes paralysis and uselessness (*lakva*).

But if the food is left undissolved and passes down through the bowels, it causes *thuqmay* (? dyspepsia) and *lopindsch* (? *gulundj* = tranchées), and looseness above and below.

But if the liver is overcome with heat, and the stomach be thrown into disorder and prevented from disposing properly of the food and from sending its contribution to its master, and if the passage for bile (*or* of the gall) be blocked, then the liver is unable to tolerate this yellow bile (*safra*), but sends it abroad into the system (*or* person) and renders it *sralan* (? *deλnuthiun*, i.e. yellow.) And if the red bile penetrates to the lung, it stabs. And if to the spleen, it swells it up, and the colour turns yellow and the belly is enlarged and a morbid condition set up. And there are various other diseases akin to this, I mean in connexion with the blood, and the black bile, which, if it remain in the liver, causes divers maladies.

And there issue from the liver five huge ducts and spread over all the members, and its central duct is what they term the basilica (*pasilik*), and the lower one is the *catholice*, which they call *aqhal*; and the upper one what they call *kifal* (cephalic). But two other ducts issue from the liver, from which it passes to the artery.

And all the ducts issuing from the liver are incessantly in motion, and the divisions of all the ducts, which come from (*or* are in) the liver, are 232. And God made Jupiter to preside over the liver and other ducts. But the liver is one of the great members, which feeds and nourishes all the members by the will of God.

12. *Concerning the formation of the Spleen.*

God made the spleen like a stable, which collects in itself all the excrement, and sundry burnt blood which there is in the liver and ducts is all

gathered into it ; for the liver contributes to it all the burnt blood. And there is a duct from the liver direct to the spleen, along which all burnt blood is sent straight to the spleen ; and a duct from the spleen goes to the heart, which contributes cold to the heart, which counteracts the heart's heat lest it burn the person. And the duct from the spleen to the liver, when the spleen fails,—then the burnt blood therefrom issues to the liver. And the liver cannot sustain its weight, but disperses it to the members of the body, and renders a man *savtakot* (? melancholic) and carbunculous and pockmarked and *quthesh*, and otherwise diseased.[1]

And the cause of all this is burnt blood, which gathers in the spleen and damages the liver and excites these complaints. For the liver cannot carry the load and disperses it into the members. But if this burnt blood goes straight to the heart, it induces melancholy and epilepsy.[2] But melancholy is of several kinds, and one sometimes laughs, sometimes weeps, sometimes is calm, speechless, and insensible.

If this burnt blood collects and goes straight to the brain, it renders a man epileptic. And this ailment has three kinds like melancholy. One as it were besets a man and draws him from aloft downwards ; another impels him as it were to stand erect and then fall down, and he foams at the mouth, and makes foul noises, and dashes foot and hand and head on the ground, and many pass urine. Another kind is that which drives a man to a spring or to water, and he will declare that good children beset him, and talk with him and beat him. And all this is caused by burned blood collecting in the spleen and overburdening it and swelling it up ; and next it causes *isthisλayilahmi*,[3] which means a tumid condition of the human system and the face grows fat ; and if red bile is mixed therewith, it produces pleuritis (lit. malady of the ribs).

God made the spleen cool and dry, and made it the chamber of black bile, and the taste is sour. And Zohal (Saturn) is appointed its presiding star.

13. *Kidneys.*

God made the kidney the seat of desire, and it was made more delicate and subtle than other seeds, and it was set firm in the middle by a sinew (*or* fibre), and from the sinew enclosed firmly in a pelt which protects [and] covers this pellicle of the kidneys so that the heat of the heart may not consume it.

And a duct of blood issues from the heart and goes to the brain, and from the brain descends along two ducts behind the ears and passes under the muscles through the one full of blood and through the other full of breath (*or* air), to the kidneys. And it descends and is cooked (*or* digested) and the water (*or* liquid) is afterwards separated and what is left becomes semen, and of it the child is engendered in the woman's womb by the command of God. The kidneys are so made that from the right and left hand one there is a passage for the water (*or* liquid). And they were made the abode of desire by God and of seed, and Lucifer appointed their presiding star.

14. *Concerning the formation of the Gall.*

God made the gall bladder the abode of bile which is red bile, because whatever of heat and dryness there would be in the person is all collected by

[1] Father Akinean renders his MSS. : ' And on the body warts are formed and eruptions are caused and pockmarks and erysipelas, and pigs (a kind of malady) and other forms of sickness.'

[2] *malas kath.* The first word elsewhere (Section 10) is interpreted phlegm. Akinean : pesanteur de tête or heaviness after drink. I believe it is μέλας, and that the words here used = μέλαινα σταγών, *atra gutta.*

[3] ? Elephantiasis. Akinean : Hydropisii.

the liver and sent straight into the gall bladder; and it has its entrance; but it has no way of issuing out. And God made its fibre (sinew) cool and moist, and its pelt (δέρμα, *corium*) hot and dry, that the cold and moisture of the fibre and the heat and dryness of the pelt may take away the bile of the liver and correct the same. For were it not so, there would be heat of the bile and it would consume the kidneys.

And its presiding star is *Marekh* (Mars).

15. *Concerning the Bladder.*

God made the bladder a vessel for the water of the entire person, so that whatever water is secreted from the stomach and liver may be collected in it.

But the bladder has two tunics. The interior tunic is the urinary receptacle (lit. water-pourer), and the outer tunic is for the semen, which descends from the kidneys along the outer tunic and proceeds to the right-hand testicle, where it is collected and then discharged by the urethra to fall into the woman's womb, where it engenders the child by divine command.

And stone is formed in the semen and is deposited in the bladder, and the urine and the stone pass out together. And there are some men who cannot pass their water; for if the vessel of the urine becomes hot and dry, the urine comes out yellow and mixed with bile, and the passage burns and blocks the mouth of the bladder.

But if the penis (?) turns hot and humid, and then it is filled, but the man cannot retain or keep the urine, and it often issues forth, and if it should happen at any time that it forcibly hinders (micturition), then the man's body suffers relaxation, and the receptacle of urine is distorted; and if for this reason (the urine) cannot any more enter (the urethra), and if no passage be found by which it can enter afresh, then [the urethra] often will burst and the man die.

God made the bladder cool and humid, and appointed the Moon to preside over it.

16. *Concerning the formation of the Testicles.*

When the testicles were constituted by the command of God, man's strength was placed in them, and they were made the place of semen and of desire. And the desire of seed goes out from them and is spread over the body and the eyesight, which desires and impels a man to gratify his passion.

And a man's beard absorbs stuff from the eggs (i. e. testicles). You can make the experiment yourself. For a man entering the bath, before he washes lays them in sesame oil; and when he quits the bath after washing, his beard becomes lustrous and soft and sleek.

And the eggs constitute the difference between man and woman: for if they are cut, a man becomes beardless, like a woman. Nor can he any more beget a child, and they are one of man's most important members. The wise Galen declares there to be four important members in a man's body, viz.: 1, the brain; 2, the heart; 3, the liver; 4, the eggs. Were they not one of the most important they would not be so allied with the heart, as you can see for yourself. If they are violently seized and insulted, a shivering comes into the heart and it faints, and unless they are quickly released, the man at once dies. And the presiding star is Lucifer or Zôhray.

17. *Concerning the Stomach.*

God made the stomach of sinew and flesh, and around the mouth of the stomach runs copious sinew, but on the inside on the floor of it copious flesh; for this reason that flesh is hot and sinew cold. And so the food is assembled in a hot place and there concocted; for in a cool place things

cannot be cooked. And the sinew was for this reason set above, in order to draw the food and drink into it. And whether the food is at one time plentiful and at another scanty, makes no difference. For however much you drag the sinew it relaxes, and when you let it go it returns again to its position ; whereas if you drag the flesh it at once is lacerated. And in the stomach are four virtues (*or* powers). The first they call *Djazip* (Arab. *pull*), because it is a dragging force, the second *masiq* (Arab. *retain*) which is retaining. The third they call *Hazim* (Arab. *digest*) or digestive, and the fourth *Tafiay* (Arab. *repel*) or expulsive, and sending out. But even if things are a thousand times light or small they require seven hours in order to be cooked (digested) and melted down. But if the stomach is hot and vigorous, and thoroughly dry or wet, food is rapidly digested before the fifth hour. But if the stomach be cold and wet, the digestion takes place otherwise. If the food be very light and defective, it is quickly digested and assimilated. And whatever one eats or drinks, the stomach assimilates it into itself and there it is digested ; for on the right side is the liver and on the left the spleen. Above it again is the heart, and below it the gall ; behind is the spine and in front the abdomen. And when the food and drink descend into the stomach then the inner mouth of the stomach shuts, so that the food and drink do not flow out until it is properly cooked and digested and its nutritive virtue appropriated, and sent on to the liver to make blood, and to be apportioned to all the members as suits them. And then it opens a passage to what is useless and conducts it out, and physicians call this the *pap* (i. e. *bab*), which is doorkeeper or pylorus.

18. *Concerning the Guts.*

God made the guts in seven parts, for that is the statement of Bagarat (Hippocrates). But Galen declares for six guts. For the first one which is contiguous with the stomach is not reckoned by him among the guts, as it is by Bagarat ; where it quits the stomach and divides off, it has another rôle than the guts, and is named the pylorus, because it discharges a different function. Moreover the six guts form groups of three, because three of them are thick and three slender.

The first thick one they call *ithnayashara*, which is twelve (*duodenum*), and another *λulin*, which is vocal. And another *musthaλim*, which is foundation and sensation of the guts. But of the slender ones, the first is called *sayim*, which means dry gut, another *avar* or crooked, and another *taλiλ* or minute. In all, three thick and three thin, apart from the pylorus which adheres to the stomach. But all food and drink taken by a man is collected in the stomach, which receives the nutritive elements as required, while the useless are presently dispatched without by the wind, for the latter descends to the lung.

*Colophon.*

This book was written by Halathzaden, an unworthy sinner, dust and ashes. Remember Astuadsatur (i. e. Theodore) the Elder, our father, and Christ remember you.

## TRACT II

19. *Concerning the formation and devising of Bones and Sinews and Ducts* (or *Veins*) *and all Blood.*

The all-wise God formed the joints of man of bones 248 in number, and articulated them together, and bound them fast with sinews, and cemented them into one body. And he set in them ducts for the vitalizing and irrigation (lit. inebriation) of the entire body.

Now in the head man has five bones, and in the entire *qnateλ* (i.e. temple) is one bone, and the teeth consist of 32 bones. The whole number of bones in the head is 39. In the neck and vertebra [1] (*or* ? tibia) are two bones.

But in the hand, i.e. upper limb, in the shoulder are two. In the *dsil* or elbow is one bone. In the upper (?) arm one bone, in the forearm two bones. In the palm of the hand 35, which makes all the articulations of the bones of the hands 41, with a corresponding number for the hand on the other side.

In the back bone and vertebrae [2] there are 18. In the right hand ribs there are nine bones, in the left hand ribs eight.[3]

And in the feet (?), in the *Djur* there are two, in the member (μέλος) one, in the knee two, in the shank (*srunq*) two, in the vertebrae of the foot 35, which makes all the bones of the foot to number 42, with a corresponding number in the other one.

But a woman has 252 bones, four more than a man, and these four bones are in the generative space of the woman, and they interlock like fingers ; and when a child is about to be born, by the command of God they divide back from one another and the child issues forth, and as soon as it is separated from the mother, they close again and join by the command of God.

But God devised the sinews (*or* nerves) of man 248 in number, first the great sinews which are bonds (*or* yokes) 25, and then are divided, and become lesser (*or* thin) ones, 228 in number. And the sinews were made by God to hold fast the bones. But the ducts of man were made by God to replenish with moisture all the members of the body.

But God also set apart in the human brain 14 ducts, and attached every duct to a member, so that all the members are sustained and nourished by them, and are invigorated by one another and fortify the person.

And the ducts send and stimulate the person to movement, and the tongue to speech, and run also to eye and ear and other senses. For four ducts lead to the eyes and move them. Two communicate light to the eyes and two move them. But these two ducts which give light are void of blood and dynamical, so that the eyes are *datzmov* (?) sustained from the brain. When these two ducts are cool and wet, a man sees well from afar and ill from near ; but when they are hot and dry, one sees well from near and ill from far. And if they are violently harmed a man goes blind.

And two ducts run to the ear and are empty and bloodless. Their rôle is to guard the ear and transmit whatever it hears to the brain. And God created the ear a hard cavity and spiral, so that a loud sound cannot suddenly penetrate, nor water, nor insect get in and reach the brain and injure it. But if these two ducts are impeded by superfluities, then the ear is rendered deaf. And two empty ducts lead to the nose, to receive sweet smells and send them to the brain, and by night they keep ready.

And God made the nose a grotto (*or* fortress) to keep continually open by night and day, and to keep wet, in order to drain away all superfluities and preserve the brain clean and pure, so that the latter may remain untroubled by epilepsy and swoon, and from paralysis and from *lakua* (? helplessness) and lunacy (? melancholia), and from headache and all other diseases of the brain.

And there is a duct in the palate of the mouth which is sensitive to all savours, sweet or bitter, salt or saltless, sharp and acid, which pull and draw the brain to the controlling principle (?).

And another duct is in the tongue, which enables the tongue to talk.

[1] *lisern*, κνήμη, or σφόνδυλος, or vertebra. [2] *lisern* above so rendered.

[3] One rib having been abstracted to constitute Eve.

And as the tongue is the hand of the jaws, which collects the food and gives it to the teeth for them to pulverize it ; in case anything escapes the teeth, the tongue collects it afresh and directs it to the teeth, like grain which is thrown with the hands between the millstones, the tongue being the hand of the jaws.

And two ducts lead to the hand, and each of these two ducts is divided into two. And one runs toward the right hand and one toward the left, and these regularly keep the hands in movement.

And two ducts go to the feet, which equally keep them in movement.

And one duct runs to the two testicles from the brain, to move them and hold them firm and enable them to fulfil its desire. It is the instrument of desire ; and if it is hindered from acting, they use drugs and infusions to re-establish and clean the duct, to enable it to function afresh.

And a woman's ducts are eight more in number than a man's, and she has four ducts which allow her to fulfil her female functions, and four in the breast whence milk issues, two to one teat and two to the other, which makes up the number of all the female's ducts to 248, by the will of the ineffable and incomprehensible God.

20. *Concerning the formation of the Seven Members, whereby man hath life ;* the first member is the brain, second heart, third liver, fourth lungs fifth reins, sixth gall bladder, seventh spleen, which makes up the seven organs in man that are not external, as are eyes, brows, nose, ear, tongue, hands, feet, testicles (eggs). For one of these may be cut off, or two or three of them, and the man survives ; whereas if a single one of these internal members on which his life depends fails, he dies irreparably.

And we are to know that perception, sensation, motion, wisdom and vigilance reside in the brain, whose nature is cool and moist. Life and light and strength and redness in the heart, whose nature is dry and hot. Joy and laughter and gladness in the liver, whose nature is hot and moist. Breath (*or* spirit) and voice and *yejul* (? *hival*) in the lung, whose nature is cool and moist. Desire and power to copulate and seed are of the reins, whose nature is hot and moist. Irritability and anger and love of power in the gall, whose nature is hot and dry. Shame and rancour and impatience and suspense in the spleen, whose nature is cool and dry. And as long as these seven members which occupy man's interior are of even tenor and free from superfluities, a man is healthy ; but if one of them chances to be injured, you may recover health with God's aid by means of wise physicians and drugs, for God has bestowed wisdom on experienced physicians so that they can make good by remedies the injury according to its nature.

But if the nature of the members gets astray and inverted, so that the brain turns dry and hot, or the heart cold and wet, or the liver cold and dry, or the lung hot and the gall cool, in such conditions of their natures, the possessor of them invariably falls sick,[1] and it is hard to heal him, nay, he can only die.

And God made in the windpipe a bit of flesh like a column, which is called *zankik* (? epiglottis or uvula), to protect the breath, so that no food or drink go astray and diverge from the passage to the stomach, and enter upon that by which the breath issues from the lungs. This going astray the epiglottis prevents. But if on a sudden food should pass along the passage by which the breath issues from the lungs and fall into the said pipe and obstruct the breath, then a man is oppressed and sick (lit. over-hot). In such a case the breath is constricted, and it is an effort to draw it ; for if it cannot find its way out, the man will die. For there is no interior passage by which

[1] *uchtavor* for *achtavor*.

it can escape, since the lung reposes (?) obliquely (?) on the stomach, and therefore has no passage from within.

And in respect of the lung physicians have said that it continually ventilates the lung ; for if the lung wing did not waft the air in from outside through the lung and so enable the heart to drive its heat without, then its heat would wax so great as to consume and burn up the system, and the man would die. On the other hand, except for the heat of the heart, which the lung receives from it, life could not stir in the man. And there are these two airs, of which the one issues out and the other enters in. The one which enters is cold and is called *pisar* (Pers. *bisear*, i.e. numerous) or *erat* or ample and copious ; but that which goes out is stifling hot and is called *khupat*, i.e. wanton or proud and potent (?).

And skilled physicians diagnose death or life by this criterion : when they lay their fingers on the duct and understand.

And they call the liver *αλsamelptan* (?), that is the purveyor of the body and feeder. For when the meat and drink are collected in the stomach and digested, the nutritive and useful elements are passed on by it to the liver which receives them and converts them into blood and sends it streaming to all the members, and so nourishes and sustains the strength of the person.

Thin and clear blood is given to the heart, but the sour (*or* hard *or* rough) element of it to the gall, while such as is coarse (*or* thick) and watery goes to the lung, and what has a moderate amount of fat in it to the reins, and the dregs of the blood goes to the spleen. But such part as has little moisture and red (*or* yellow) bile goes to the bowels. For ill-digested and unwholesome matters descend into the gut from the stomach, for the bile to rarefy them. They are discharged for convenience, and any red bile or food becomes a superfluity in the liver, and descends readily into the guts.

And as long as the liver is warm and wet, it is vigorous of function and quick to digest and quick to pass on ; but if it be enfeebled, then the liver at once cannot digest and pass the food on. And this unconcocted blood remains in the gut and becomes scurf (*or* scab) and induces colic. And should pale bile increase in the liver and engender blood that like a fibre exists there and moves, the man will take too little nutriment and nature will not assimilate the food. And pale bile cannot give blood to the gall, because there is (only) enough of it in the liver, and it would cause harm and disease in the man.

But when there is too much of phlegm (*pituita*) in the liver, so that it cannot send blood to the gall, as in the case of the pale bile we wrote of above, in this case it is badly liquefied and becomes water and sets up pains of *sokhay* (ill nutrition), so that the body swells. And when the black bile finds no passage to the spleen and the road is blocked, that gives rise to *Hillath* (Arab. languor), like pox, etcetera.

And of all the diseases which affect man, all the causes can be traced to the liver being feeble and unable to send on its superfluities (? secretions) to their places, and to distribute them to the members. For the health and life of the whole person is in the liver, by the care of God.

21. And now, brethren, our original was very imperfect and faulty, and we were barely able to bring to so much accuracy as this. And you that are aided by it, bear in mind the much-sinning servant, and may God have mercy on you.

## TRACT III

### 22. *The Parts of the Body.*

The brain is a part of the body containing the bond (or conjunction) of life. Bone is the casing of the brain. Sinew is the (instrument)

of the body in movement. The duct (or vein) is the (instrument) of the blood. Blood is the nutriment of the body. The breathing is the (instrument) of spirit. Spirit is the (instrument) of prime movement. The belly is the instrument of primal fecundity (or enjoyment). The liver is the instrument for the replenishment of the veins with blood. The heart is the origin of living (or psychic) life or origin of the life of the soul. The soul is of one breath. The hands are instruments in giving and taking. The feet are instruments subservient to going one's way. The mouth is an instrument of voice and of taking food. The tongue is an instrument of speech and of taste. The sense organs are instruments of sensation. The eye is an instrument of seeing ; the ear of hearing ; the nose of smell.

Body is the instrument of touch. Bile is a refinement (*or* subtilizing) of blood. *Malas* (? = phlegm, *pituita*, mucus) is a refinement of bile. *Mamatz* (chyle) is a superfluity from first food. Seed is moisture with breath in transmutation of blood suitable unto life or a superfluity of the last food agreeable, and resembles a creative agency that begets him, or a nutritive soul (?). Creation and birth, or the first food of a living being in transmutation of body.[1]

And glory be to Christ, God our hope.

It is to be remarked that there are three sources (or beginnings, fundamentals), to wit, the cerebellum (or meninx) whence spring the sinews (*or* nerves) which run together to the neck, pass to the throat and divide towards the shoulders, hands, and rest of the body.

This nerve (*or* sinew) binds together and secures all the joints of the bones, and gives play and movement to the feet and hands and all the animal members. . . .

But the heart is the source (*or* principle or foundation) of the arteries ; for it possesses two chambers on the right side to collect the blood, which it takes from the liver, and on the left side it contains air collected, which it takes from the lung. And on this side it has four ducts without blood, full of warmth, and it divides into 32 several ducts, which makes up 108 ducts, by means of which is distributed warmth and life (over) the entire body. And these ducts are called *madjas* (? μέλος).

And they reveal disease and health in man. But the liver is the source of the blood of the ducts that issue from it, five in number full of blood, and three of them of great size. And we call the uppermost duct *Kifal*, and the middle one *Basilica*, and the lower one *Akhal* (?). From these branches off a duct from the forearm. And all the ducts from the liver separately number 232, and by them blood is transmitted all over the person.

But be it remarked that the body is attended by these three, to wit, sinew (or nerve), artery, and blood duct. For strength descends from the head by means of sinews (*or* nerves), and distributes it to all the body and so affords sensation ; and the blood feeds and strengthens, and an artery affords warmth and life to the entire body.

And the much sinning Halathidj the Elder ye shall remember, and may God remember us in the day of judgement.

## TRACT IV

### 23. *Concerning the Human Organism.*

Of the great Gregory of Nyssa.
An abridgment.

§ 3. Man is said to be of four constituents, for there are four causes of the genesis of all beings, fire and air, water and earth. And water is

[1] The last lines concerning seed are unintelligible.

a seminal nature, and generative after the semblance of woman. But fire is energizing after the semblance of nature; for two of the elements are creative or active, fire and air, and two are passive, water and earth. For fire and air are male, while water and earth are female; because the humidity of water without warmth of fire is barren, whereas warmth of fire and humidity of water, intermixed, are fruitful for generation.

The first synthesis (?) then of the four elements is fire; because more subtile and ever moving and nimbler is the nature of fire, like the nature of desire in us which they call an irrational movement, i.e. passionate (*θυμικόν*) and concupiscent, whereby the living (being) is released, for without these it is impossible for the living being to be released and constituted.

The first movement then is desire on the part of the fire in us, by which one approaches a woman, and the instruments of the seed are two ducts which descend from the head to the reins and lead to the groin, the one duct is full of blood, the other is full of air; and when the duct of blood reaches the groin, the blood is turned into seed, as in the breasts of women, where blood is turned into milk. And when one approaches a woman, the seed falls into the woman's womb, in the same way as seed of plants which falls into the earth. And in turn the seed of the woman, like milk in a vessel, is something warm, and the mingled seed becomes for her like the curds of milk, and in the union of seeds they are clotted with each other and form the foetus, and thereupon is fulfilled the word of Job which says: 'Like milk thou milkedst me and like cheese thou didst coagulate me.' And there is fulfilled the prophet's word which says: 'The two shall be made into one flesh', for out of the seed of man and woman is made the child, out of the twain one flesh made substance, like a river divided into two streams and again united in one. Now by the coagulation of the seed in the woman's womb it is turned back into blood during six days, and on the sixth day there is fashioned the worm, as the prophet says, 'I am a worm and am not a man.' For the first man was only completed on the sixth day and moulded by the hands of God, but now it is from corruption and from passions that he is engendered. And on the fortieth day members are completed for him, and then he is created the spirit of God, as to whom he alone knows who created him; as he said: 'The spirit shall go forth from me and all breaths I made' (? from myself). But a female child is completed in 70 days and then receives the spirit. For a male child is assigned to the right side of the mother and the liver and gall are on the right side, and are hot and easily make the child to grow, as a flat country with vines quickly matures the fruit. And a female child is assigned to the left side of the mother, and there is the spleen, which is cold. For that reason the child's growth is long, like fruit on a mountain which ripens late.

But in the moulding of a child, according to the nature of the parents it comes into being; according as they are diseased or healthy, so is constituted the alloy of flesh. Wherefore it is necessary for a man and woman to choose a time when they are untainted by sickness, for then their child will be healthy, like husbandmen who choose a time for sowing. For if the ground is dry, or again too muddy and wet, the seeds are damaged and give no healthy fruit. How much more he who sows rational seed for the engendering of children in rational soil? Wherefore it is necessary to make good choice, that the child may be healthy and free from all complaints. But when the child begins to sprout in its mother's womb it receives food through the umbilicus and grows like a melon or a gourd which receives water through its stalk, etc.

# STEPS LEADING TO THE INVENTION OF THE FIRST OPTICAL APPARATUS

By Charles Singer

The story of the early stages in the evolution of our two most important optical appliances, the *microscope* and the *telescope*, is essentially one and can only be told as one. With the construction of working models of these two instruments their history begins to diverge and can be treated separately.

The microscopes and the microscopic work of the classical observers, Leeuwenhoek, Malpighi, Hooke, and Kircher, have frequently been described, and have been discussed by the present writer in two previous articles.[1] In one of these can be found also a description of some of the microscopical work of the first Accademia dei Lincei (1603–30) and of its early members, Federigo Cesi (1585–1629), Francesco Stelluti (1579–1651), Fabio Colonna (1567–1650), and Joannes Faber of Bamberg (1570–1650). These men all died before the work of the classical microscopists had begun.

Better known are the labours of the earliest scientific designers and constructors of the telescope, Galileo and Kepler, whose work, like that of the earlier microscopists, was completed in the first half of the seventeenth century. The most important documents relating to the discovery of the telescope are printed in the pages of the monumental *Edizione nazionale* of the works of Galileo edited by Professor Favaro. In English the story is told in the older *History of Physical Astronomy* by Robert Grant, in an article in the present volume, and in an essay by G. Moll in the first volume of the *Journal of the Royal Institution*. Special mention must also be made of the remarkable achievement of J. Hirschberg, whose *Geschichte der Augenheilkunde* is a permanent monument of its author's skill, learning, and industry, and is of far wider scope than its title suggests.

In the pages which follow we push the inquiry further back and set forth in chronological order the actual advances in the practical knowledge of optics which rendered possible the construction of the earliest compound optical instruments. These

[1] Charles Singer, 'Notes on the Early History of Microscopy', *Proceedings of the Royal Society of Medicine*, 1914, vol. vii (*Section of the History of Medicine*), pp. 247–79, and 'The Dawn of Microscopical Discovery', *Journal of the Royal Microscopical Society*, 1915, pp. 317–40. These papers contain bibliographical notes on the material in question.

advances were more closely linked, in certain stages, with progress in ophthalmic anatomy than has usually been recognized, and we dwell on this aspect in some detail. On the other hand, the account of the last stage in our story, the work of Galileo and Kepler, we pass over briefly as it is more readily accessible.

1. Among *writers of classical antiquity* there are a number of references to *burning-glasses.* It is not always clear whether a concave mirror or a crystal ball or a glass globe full of water is intended. The earliest of these references is that of Aristophanes in the *Clouds* (*c.* 424 B.C.), which runs as follows:

*Strepsiades.* I have found a very clever method of getting rid of my suit, so that you yourself would acknowledge it.

*Socrates.* Of what description?

*St.* Have you ever seen this stone in the chemists' shops, the beautiful and transparent one, from which they kindle fire?

*Soc.* Do you mean the burning-glass?

*St.* I do. Come, what would you say, pray, if I were to take this, when the clerk was entering the suit, and were to stand at a distance, in the direction of the sun, thus, and melt out the letters of my suit?

*Soc.* Cleverly done, by the Graces.

*St.* Oh! how delighted I am, that a suit of five talents has been cancelled.

Another unmistakable reference to the caustic power of such globes is that of Pliny the elder (A.D. 23–79), who says that 'if one holds glass globes containing water (*cum addita aqua vitreae pilae*) against the sun they give out such heat that they burn clothes'.[1] The most unequivocal statement that the magnifying power of these globes was known is perhaps that of Seneca, who wrote (*c.* A. D. 63): 'Letters, however small and dim, are comparatively large and distinct when seen through a glass globe filled with water'.[2] That these burning-glasses were sometimes made of solid crystal we again learn from Pliny, who states, 'I find among doctors that there is nothing thought better for cauterization of the human body than a crystal ball (*crystallina pila*) placed against the rays of the sun'.[3]

Among the best known of the passages in classical writings that are alleged to describe lenses is Pliny's famous reference to Nero's emerald. Some have thought that the stone was cut as a concave lens and thus enabled the Emperor, who had weak

[1] *Historia naturalis*, xxxvi. 26 [67].
[2] *Quaestiones naturales*, i. 6.
[3] *Historia naturalis*, xxxvii. 2 [10].

sight, to see more clearly, but for this there is no real evidence. It would rather seem that it was used as a mirror. Nothing can really be made of the passage, but for what it is worth we give it here: ‘When an emerald is of flattened shape (*corpus extensum*) it will render the image of an object against which it is held as well as a mirror, and Nero the Emperor was wont to view the combats of gladiators by means of an emerald’.[1] It should be added that the evidence is that Nero was hypermetropic or astigmatic, not myopic, and that his difficulty was with near not distant objects, so that he would not be helped by a concave lens.

In works on optics reference is sometimes made to a so-called ‘convex lens’ of rock crystal found by Layard at the ruins of the palace of Nineveh. The object is now in the British Museum, and critical examination has shown that the theory of its lenticular nature is untenable. It is probably of the nature of a jewel or ornament, since its surface is cut in small facets and not ground smooth. If used as a lens its power would be about ten dioptres.

From a number of sites of classical antiquity crystal balls have been recovered and these may or may not have been used as burning-glasses. The point is doubtful, but it is certain that they are not lenses in the usual sense of the word. It may further be added that there is no unequivocal reference in all ancient literature to a lens in our sense, except perhaps in connexion with the anatomy of the eye. Even here the idea is absent from the Hippocratic collection (sixth to fourth century B.C.) and from Aristotle (fourth century B.C.), and it can scarcely be said to have emerged even in Celsus (first century B.C.) who, having described the parts of the anterior chamber of the eye, tells us that, ‘under these is a drop of humour resembling the white of an egg, from which proceeds the faculty of vision. By the Greeks it is called *crystalloides*.’[2] In this term he includes the structure which we now call the *lens*.[3]

2. *Euclid* (third century B.C.), in his *Optics*, considered that light passed in straight lines. Like nearly all the ancients he considered an essential factor in vision was something proceeding *from* the eye. Thus he opens his work: ‘We assume that the

[1] *Historia naturalis*, xxxvii. 5 [16].

[2] Celsus, *De Medicina*, vii. 7.

[3] Many works on Optics contain reckless statements on the optical knowledge and optical apparatus of the ancients. These are effectively dealt with by T. H. Martin, ‘Sur des instruments d’optique faussement attribués aux anciens par quelques savants modernes’, in the *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*, vol. iv, Rome, 1871.

visual rays proceed in straight lines, leaving a certain space between them. The figure formed by the visual rays is a cone having its apex at the eye and its base at the object seen. Only those things are seen on which the visual rays impinge, and those things are not seen on which they do not impinge.' [1] The Euclidian origin of this work was long disputed. By some the author was thought to be Theon of Alexandria, who lived in the fourth century A. D., and was perhaps the father of Hypatia, but the work is now believed to be a genuine product of Euclid.[2] In addition to the *Optics* of Euclid there are two other works, entitled *Catoptrica* or *De Speculis*, that were circulating in his name in a Latin form during the Middle Ages. These were certainly spurious. One was translated from Greek and the other from Arabic ; the Greek work is almost certainly by Theon.[3]

3. *Cleomedes* (first half of first century of Christian Era), in his *Cyclical Theory of Meteors*, made a beginning of the study of refraction. He referred to the bent appearance of rods partially immersed in water, and he knew that an object lying in an opaque basin, and just obscured by the brim, could be rendered visible by pouring in water. He applied the same principle to the atmosphere and suggested that the sun, even where below the horizon, might, under certain circumstances, be visible. It is remarkable that he failed to give a practical application to this view of atmospheric refraction, for he rejects, as incredible, statements of his predecessors that in certain eclipses the sun seems to be still above the horizon while the eclipsed moon rises in the east.[4]

[1] The text of the *ὅροι* here translated runs as follows: Ὑποκείσθω τὰς ἀπὸ τοῦ ὄμματος ὄψεις κατ' εὐθείας γραμμὰς φέρεσθαι διάστημά τι ποιούσας ἀπ' ἀλλήλων. καὶ τὸ μὲν ὑπὸ τῶν ὄψεων περιεχόμενον σχῆμα εἶναι κῶνον τὴν κορυφὴν μὲν ἔχοντα πρὸς τῷ ὄμματι, τὴν δὲ βάσιν πρὸς τοῖς πέρασι τῶν ὁρωμένων. καὶ ὁρᾶσθαι μὲν ταῦτα, πρὸς ἃ ἂν αἱ ὄψεις προσπίπτωσιν, μὴ ὁρᾶσθαι δέ, πρὸς ἃ ἂν μὴ προσπίπτωσιν αἱ ὄψεις.

[2] The *Optics* of Euclid has recently been translated by G. Ovio, *L'Ottica di Euclide*, Milan, 1918. The definitive edition is that of J. L. Heiberg and H. Menge, *Euclidis Opera Omnia*, Leipzig, 1883–95, vol. vii, 1895, *Euclidis optica, opticorum recensio Theonis, Catoptrica cum Scholiis antiquis*. Interesting information concerning this work of Euclid may be found in J. L. Heiberg, *Literargeschichtliche Studien über Euclid*, Leipzig, 1882, and T. L. Heath, *The Thirteen Books of Euclid's Elements*, Cambridge, 1908.

[3] It is printed by Heiberg and by Ovio (see above). The Arabic work is discussed by S. Vogl, 'Über die (Pseudo-) Euclidische Schrift de Speculis,' in the *Archiv für Geschichte der Naturwissenschaften und der Technik*, i, p. 419, Leipzig, 1909.

[4] The work of Cleomedes has been edited by H. Ziegler, Leipzig, 1891. Some valuable notes on his work are to be found in T. L. Heath, *Aristarchus of Samos*, Oxford, 1913.

4. *Rufus of Ephesus* (flourished about A.D. 100) improved on the description of the eye by Celsus, and for the first time in literature, so far as we know, refers to the lens as 'lentil shaped' (φακοειδής). The passage occurs in the work *On the naming of the parts of the human body*, and runs as follows: 'The fourth tunic encloses the crystalline humour (τὸ κρυσταλλοειδὲς ὑγρόν); at first this had no

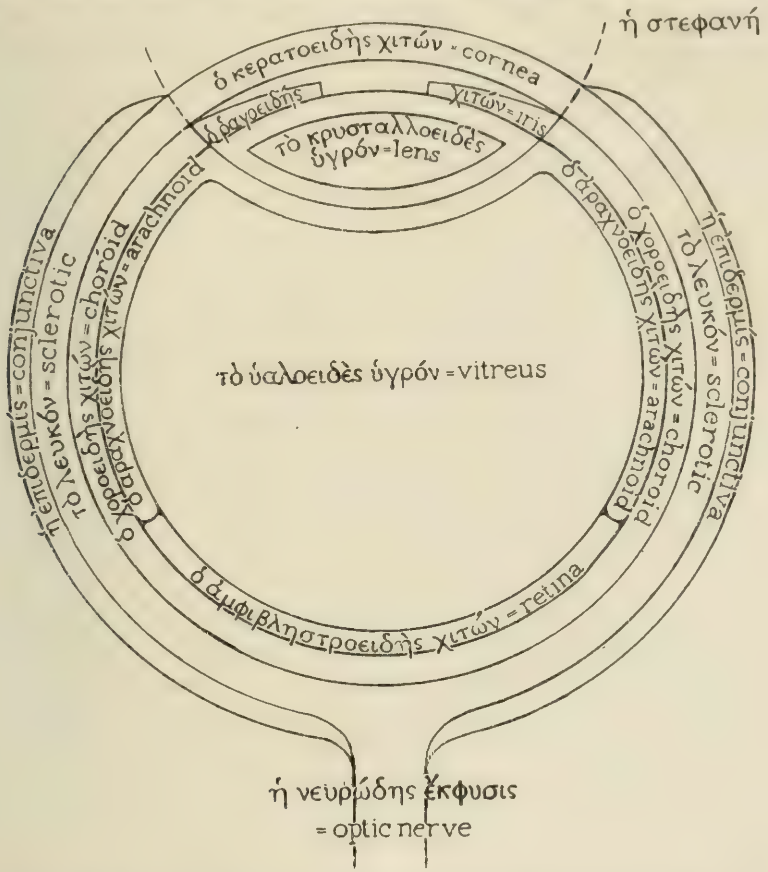



FIG. 1. THE STRUCTURE OF THE EYE RECONSTRUCTED FROM THE DESCRIPTIONS OF RUFUS OF EPHESUS (FIRST CENTURY C.E.)

special name, but later it was named *lentil-like* (φακοειδής) on account of its form, and *crystalline* on account of the character of its humour (κρυσταλλοειδὴς δὲ διὰ τὸ ὑγρόν).' Elsewhere he refers to it as 'disk shaped' (δισκοειδής).[1] The description of the eye given by Rufus is distinct enough for a diagram representing his views to be reconstructed (Fig. 1). The work of Rufus was, however, unknown to the Latins of the West in the Middle Ages who copied

[1] The works of Rufus of Ephesus are available in an edition by C. Daremberg and E. Ruelle, Paris, 1879.

and modified the less accurate description of Galen (131–201). That view was barren and we need follow it no further. But the works of Rufus became known to the Arabian writers—largely through the influence of Rhazes (860–932)—and among them (see sections 7 and 8) we shall trace a profitable line of optical advance.[1]

5. *Hero of Alexandria* (flourished second half of first century) advanced the knowledge of optics from the experimental side. He showed in his *Catoptrika* and his *Dioptra* that light is reflected from a surface at an angle equal to the angle of incidence.[2] His instrument, the Dioptra was, in effect, a theodolite and depended for its working on the principle of the equality of these angles. The *Dioptra* was unknown in the West until quite modern times,[3] but the Arabian optical writers were acquainted with it much earlier.[4]

6. *Ptolemy* (died about A. D. 155), in his *Optics*, continued the study of refraction begun by Cleomedes, and applied the experimental method to this subject. He showed that luminous rays, in passing from one medium to another, are deflected, and he attempted to measure the deflection. This work of Ptolemy was written in Greek, and has been lost. It was translated from Greek into Arabic and, in the twelfth century, from Arabic into Latin. Only the Latin version survives, and its attribution to Ptolemy is doubtful.[5] This work was exceedingly important for

[1] The theories of the structure of the eye among Greek writers may be examined in a series of diagrams prepared by H. Magnus, *Die Anatomie des Auges in ihrer geschichtlichen Entwickelung*, Breslau, 1906. Some of these figures need revision. The propagation of Galen's ophthalmic anatomy in the West and its progressive deterioration is traced in a group of learned contributions by K. Sudhoff, extending over a number of years, in the *Archiv für Geschichte der Medizin*.

[2] The works of Hero are edited in the Teubner series by Wilhelm Schmidt and Hermann Schöne, *Heronis Alexandrini opera quae supersunt omnia*, Leipzig, 1899–1903. There is an unusually able and exhaustive but anonymous account of Hero in Pauly-Wissowa's *Real-Encyclopädie der klassischen Altertumswissenschaft*.

[3] The text of the *Dioptra* of Hero was first made known in the West by a translation by L. Venturi in his *Commentari sopra la storia e la teoria dell'ottica*, Bologna, 1814. The earliest manuscript was discovered by Minoïdes Mynas on Mt. Athos in 1841, and is of the eleventh century (Paris, Bib. Nat. Supp. gr. 607).

[4] The knowledge of Hero displayed by the Arabian writers is discussed by E. Wiedemann, *Sitzungsberichte d. phys.-med. Soz. zu Erlangen*, xxxvii–ix.

[5] G. Govi, *L'Ottica di Claudio Tolomeo da Eugenio Ammiraglio di Sicilia . . . ridotta in Latino sovra la traduzione Araba di un testo Greco . . . conforme a un codice . . . Ambrosiano*, Turin, 1885. See also A. Favaro, 'L'Ottica di Tolomeo', in the *Bullettino di Bibliografia e di Storia delle Scienze matematiche e fisiche*, xix, Rome, 1886.

its influence on mediaeval optics both in the East and West, and Alhazen particularly was dependent upon it.[1]

Greek science in its creative aspect practically expired with the second century. Its last optical work is that of Ptolemy, unless we include the treatise that has been attributed to Theon (see 2, above), in which case we must consider it as prolonging a feeble life until the fourth century. We have seen, moreover, that among the ancients there is no definite evidence of knowledge of lenses apart perhaps from the lens of the eye. But even had lenses been known they could hardly have been clearly described, for the prevalent view, popularized by Plato, that regarded vision as something *emanating from* the eye, would have prevented appreciation of their nature.

7. The fall of Greek science was followed—after a long interval—by the rise of the Arabian system which was based on it. The earlier Arabian optical writers, Jacob al Kindi (750–850)[2] and Costa ben Luca[3] (864–923), for example, were concerned only with the translation and codification of Greek works of science. More important for our purposes are certain later Arabian philosophers who were led to abandon the Platonic theory of emanation and were thus in a better position to appreciate the nature of the eye. The most important of these was Alhazen.

*Alhazen*, or Al Haitham (Ḥasan ibn al Ḥasan ibn al Haitham, Abu Ali, 965–1038), was an Arab of Basra, who abstracted the work of the older Greek optical writers but exhibited some originality. He devoted much space and skill to the development of the optical effects of curved mirrors. He had a fairly clear notion of the nature of refraction, and improved the apparatus of Ptolemy for measuring the angle of refraction in different

[1] See S. Vogl, *Roger Baco*, Erlangen, 1906, p. 26.

[2] Al-Kindi = Jaqub ibn Ishaq ibn al Sabbah, al-Kindi, abu Jusuf wrote many astronomical and mathematical works including one, *De speculis ustiris* that has not been rendered from Arabic unless it is represented by the Bodleian MSS. Digby 168 (XXXVI), fol. 129, *Alkindi ex libro perspectiva* or Canon. Codd. Misc. 370 (II), fol. 240–end, *Alkindi liber de radiis stellatis*. See, however, Heinrich Suter, 'Die Mathematiker und Astronomen der Araber', in the *Abhandlungen zur Gesch. der math. Wissenschaften*, X, Leipzig, 1900. In the same journal (XXVI, 1912) is an article by A. A. Björnbo, 'Alkindi Tideus und Pseudo-Euclids drei optische Werke', including a work of Alkindi, *De causis diversitatum aspectus*.

[3] Costa ben Luca also wrote a work, *De speculis ustivis*. This does not seem to have been translated; cf. Ferdinand Wüstenfeld, *Geschichte der arabischen Aerzte und Naturforscher*, Göttingen, 1840, p. 49, and Moritz Steinschneider, *Die hebräischen Uebersetzungen des Mittelalters*, Berlin, 1893, §§ 310, 312, 336, and 342.

media. As regards his knowledge of the structure of the eye, not only was his ophthalmic anatomy better than that of most of his predecessors (Fig. 2), but he considered that vision resulted from rays coming to the eye from the object, and opposed the view, current up to the eighteenth century, that explained vision as involving the emanation of something from the eye. He placed the lens or *humor crystallinus* in the centre of the globe of the eye, and considered that in it the external impulse became converted into the sensation of vision. He speaks also of the enlarging power of a segment of a sphere of glass. The optical work of Alhazen, which is of enormous proportions, was translated into Latin by an unknown writer of the later twelfth or earlier thirteenth century.[1]

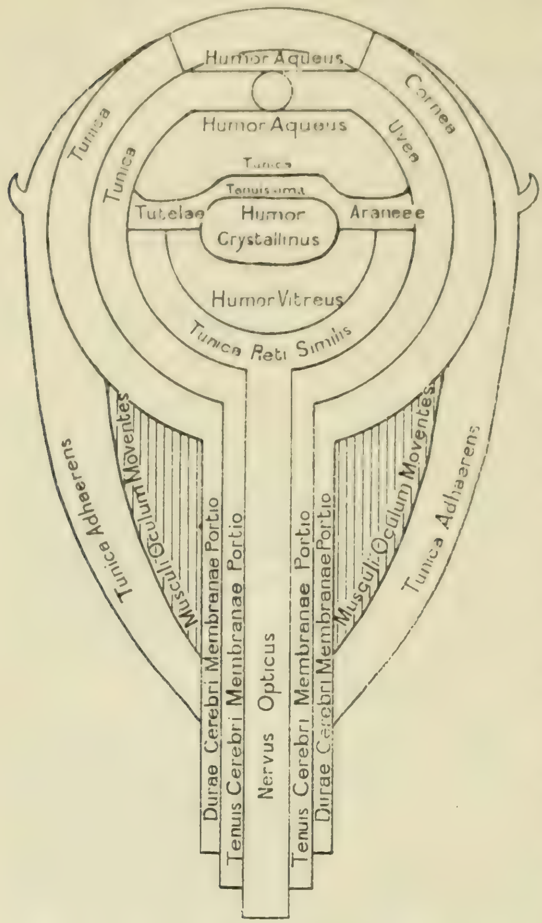



FIG. 2. THE STRUCTURE OF THE EYE, AFTER ALHAZEN

Redrawn from the printed edition, Bâle, 1572. The diagrams of the eye in MSS. are identical with this. The best MS. seen by the writer is of the thirteenth century in the Crawford Library of the Royal Observatory at Edinburgh. The diagram of the eye in it is very similar to that here presented.

8. *Avicenna* (980–1037). An important writer on Optics, contemporary with Alhazen but even more afflicted with the *verbositas arabica*, was the encyclopaedic Bokhariote, Avicenna. His views on optics are diffused through a perfect multitude of writings, but a relevant aspect of his teaching is succinctly expressed in a comparatively small work *On Physics*, which has not been translated in full from the Arabic. The passages on vision have, however, been rendered into German, from which we retranslate as follows :

[1] The work was printed by F. Risner at Bâle in 1572. A valuable discussion

'The organ of sight is the crystalline humour [lens] in the pupil. They are wrong who think that the act of seeing arises from something that goes out from the eye to the perceived object and collides with it. For if this something be material the difficulty would be that in the eye would have to be a body big enough to spread over half the vault of heaven.' He then goes on to say that the emanation theory has great difficulty in explaining the less apparent size of distant objects, 'for if impressions take place through contact [of the emanation], then the impression would be of a size exactly the same as the actual size. But if on the other hand the impression takes place by means of a flow *towards* the crystalline humour, we maintain that the more distant object would then appear the smaller. The proof is as follows (Fig. 3):

'Let the circle $E F$ be the lens around the centre $G$, and let $A B$ and $C D$ be two objects of equal size. . . . Then the triangles $A G B$, $C G D$ have an equal base but $C G D$ is taller. Further, the angle $C G D$ subtends the arc $H K$, and the angle $A G B$ the arc $L M$, so that the arc $L M$ is greater than the arc $H K$. The optical picture of $A B$ imprints itself on $L M$, and that of $C D$ on $H K$, and so it is that the optical picture of the further objects imprints itself as smaller.' [1]

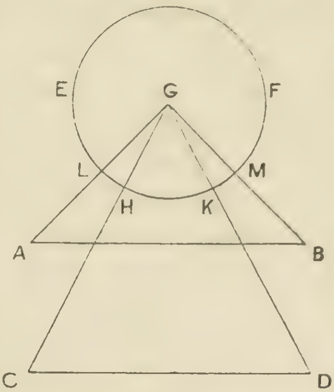



FIG. 3.

Avicenna's diagram to explain the less apparent size of distant object.

In the Qanûn of Avicenna there is a description of the Anatomy of the Eye. This only concerns us here in that the lens is regarded as flattened, instead of simply spherical as in most mediaeval

of Alhazen's work by one who is both an expert physicist and an Arabic scholar is contained in E. Wiedemann, 'Zu Ibn al Haitams Optik', in the *Archiv für die Geschichte der Naturwissenschaften und der Technik*, iii, p. 1, Leipzig, 1910.

There are a number of good MSS. of the Latin translation of Alhazen. Those known to the present writer are:

Corpus Christi College, Oxford, CL, fo. 1–114 v, thirteenth century.
Crawford Library, Royal Observatory, Edinburgh, thirteenth century.
Trinity College, Cambridge, 1311, fo. 1–165, thirteenth century.
Peterhouse, Cambridge, 209, fo. 1–111, fourteenth century.
British Museum, Sloane 306, fo. 1–175, fourteenth century.
British Museum, Royal 12 G17, fo. 102v–end, fourteenth century.
Bodleian, Ashmole 424, fo. 3–355 fourteenth–fifteenth century.
Bodleian, Digby 104, fo. 80–end, fifteenth century.

[1] E. Wiedemann, 'Ibn Sinas Anschauung vom Sehvorgang', *Archiv für die Geschichte der Naturwissenschaften und der Technik*, iv, p. 239, Leipzig, 1912.

descriptions, both Eastern and Western. The Anatomy of the Eye, as set forth by Avicenna, was conveyed to the West in the Latin translation of the Qanûn by Gerard of Cremona, executed towards the end of the twelfth century.[1]

9. *Witelo* (first half of the thirteenth century) was a Pole, who made an extensive study of the work of Alhazen in its Latin translation.[2] He was also familiar with the views of Avicenna. His own work grew out of these writers and is perhaps an improvement on them. Thus he drew up a table of refractions for three media—air, water, and glass—from which it could be seen that the angle of refraction did not vary directly according to the angle of incidence. It is doubtful, however, to what extent these tables were original or the result of direct observation. Witelo knew also, through Alhazen, of the magnifying power of sections of glass spheres, and expressed the wish that he possessed one of these in order to examine small objects.[3]

10. *Roger Bacon* (1214–94) accomplished real advances in the knowledge of optics. His work was based primarily on Latin translations of Arabian writers, and especially on Witelo's development of the views of Alhazen and Avicenna. He is distinguished from his predecessors, however, by his clear conception of the value of experiment, and by the evidence in his works that, having made a serious and continuous effort to discover the laws of refraction and reflection, he sought to apply his knowledge to the improvement of the power of vision. In this he is a real pioneer, and is in the truest sense the father of microscopy. Moreover, in the writings of Bacon there emerges a clearer though still inaccurate conception of the nature of a lens. He seems to think that there is only one refraction in the case of a lens but two in a spherical burning glass, and it is clear that the lenses he used were sections of spheres and were thus plano-convex.

[1] There are a large number of editions of this vast work, of which the first is probably that of Milan, 1473. The passage on the Anatomy of the Eye is in Book III, Fen III, chapter i, and it is translated direct from the Arabic in P. de Koning's *Trois traités d'anatomie arabes*, Leiden, 1903, p. 660.

[2] Not, as is sometimes asserted, from the Arabic. It is practically certain that Witelo knew no Arabic.

[3] The works of Alhazen and of Witelo were printed together by F. Risner at Bâle in 1572. An account of all that is known of Witelo, together with parts of his *Perspectiva* reprinted from the manuscripts, has been set forth by Clemens Bauemker in his *Beiträge zur Geschichte der Philosophie des Mittelalters*, Munich, 1908.

It is easy to exaggerate the claims of Bacon, and the wildest statements are often made about his discoveries. There is no evidence that he ever made a telescope nor any but a simple microscope, but he had an idea of the nature and property of lenses, and, groping with the instinct of genius, he did vaguely foresee both telescope and compound microscope.

The following passages will serve to indicate the stage he had reached in optical knowledge. It will be observed that in the first of the passages Bacon figures and refers to the *object* as though

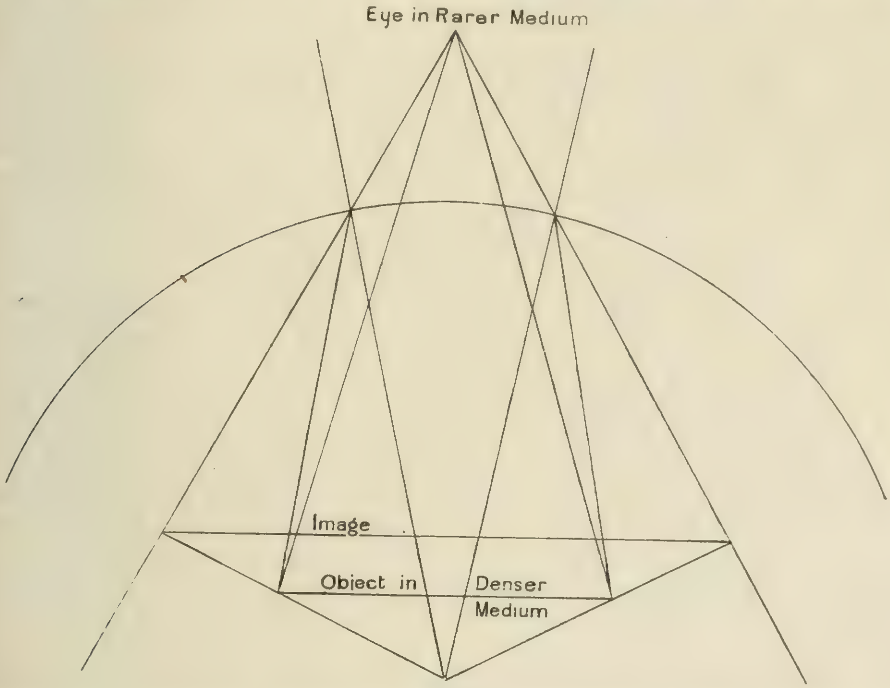



FIG. 4. DIAGRAM OF ROGER BACON TO ILLUSTRATE OPTICS OF LENS

it were itself in the denser medium of which the lens is composed. He thus confuses the optical action of the lens with that of a liquid in which an object is immersed. Perhaps he is led to do this by his knowledge that the optical results of immersion in a liquid had been investigated by his predecessors, or perhaps by their descriptions of the process of vision as taking place within the supposed central crystalline sphere of the eye. It is also apparent that he does not realize the refractive action of the *plane* surface of his plano-convex lenses.

'If any one examine letters or other minute objects through the medium of crystal or glass or other transparent substance,

if it be shaped like the lesser segment of a sphere, with the convex side towards the eye, and the eye being in the air, he will see the letters far better, and they will seem larger to him. For, according to our Canon (Fig. 4) concerning a spherical medium beneath which the object is placed, the centre being beyond the object, the convexity being towards the eye, all causes agree to increase the size, for the angle in which it is seen is greater, the image is greater, and the position of the image is nearer, because the object is between the eye and the centre. *For this reason such an instrument is useful to old persons and to those with weak eyes, for they can see any letter, however small, if magnified enough.* But if a larger segment of a sphere be employed, then, according to our Canon (Fig. 5) the size of the angle is increased, and also the size of the image, but propinquity is lost because the position of the image is beyond the object, the reason being that the centre of the sphere is between the eye and the object seen. Therefore such an instrument is not of so much use as the smaller portion of a sphere.'

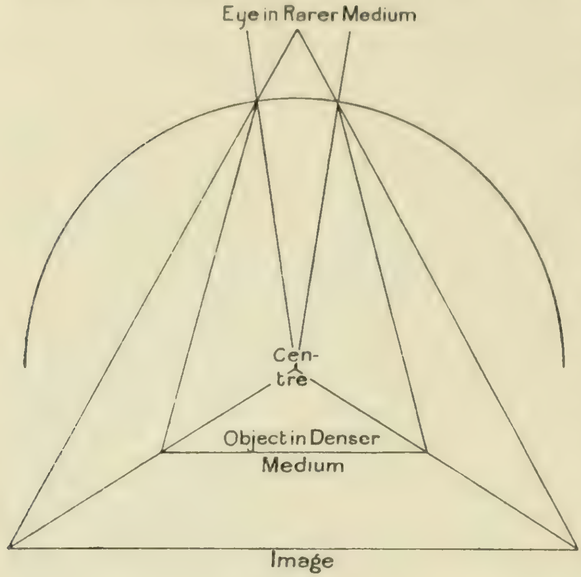



Fig. 5.
DIAGRAM OF ROGER BACON TO ILLUSTRATE OPTICS OF LENS

'Objects are greater when the vision is refracted; for it easily appears by the above-mentioned canons that very large objects may seem to be very small and conversely, and those at a great distance away may seem very near and conversely. For we can so form glasses and so arrange them with regard to our sight and to objects that the rays are refracted and deflected to any place we wish, so that we see the object near at hand or far away beneath whatever angle we desire. And so we can read the smallest letters or count grains of sand or dust from an incredible distance owing to the magnitude of the angle beneath which we see them, and again the largest objects close at hand might be scarcely visible owing to the smallness of the angle beneath which we view them; for *it is on the size of the angle on which this kind of vision depends, and it is independent of distance save per accidens.* So a boy can appear a giant, a man seem a mountain, and in any size of angle whatever, for we can see a man under as

large an angle as though he were a mountain and make him appear as near as we desire. So a small army might seem very large, and though far away appear near, and conversely: *so, too, we could make sun, moon, and stars apparently descend here below,* and similarly appear above the heads of our enemies, and many other similar marvels could be brought to pass, that the ignorant mortal mind could not endure the truth.' (*Opus Majus*, Part V.)

'As to double refraction, what is causally manifest with regard to it we can verify in many ways by the results of experiment. For if any one hold a crystal ball or a round urinal flask filled with water in the strong rays of the sun, standing by a window in

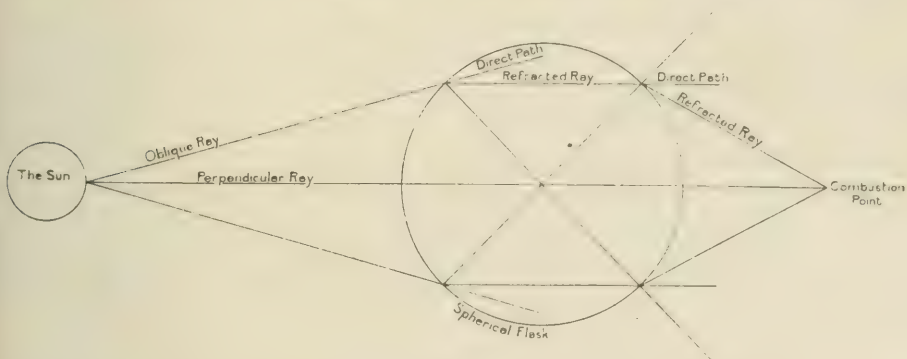



FIG. 6.

DIAGRAM OF ROGER BACON TO ILLUSTRATE OPTICS OF BURNING-GLASS

face of the rays, he will find a point in the air between himself and the flask at which point, if any easily combustible substance is placed, it will catch fire and burn, which would be impossible unless we suppose a double refraction. For a ray of the sun coming from a point in the sun through the centre of the flask is not refracted, because it falls perpendicularly on flask, water, and air, passing through the centre of each (Fig. 6). . . . But all the (other) rays given forth at the same point in the sun from which this perpendicular ray comes are necessarily refracted in the body of the flask, because they fall at oblique angles, and since the flask is denser than air, the refraction passes between the straight path and the perpendicular drawn from the point of refraction to the centre of the flask. And when it passes out again into the air, then, since it comes upon a less dense body, the straight path passes between the refraction and the perpendicular drawn from the point of refraction, so that the refracted ray may fall upon the first perpendicular which comes without refraction from the sun. *Now, since an infinite number of rays are given off from the same point of the sun, and one only falls perpendicularly on the flask, all the others are refracted and meet at one point on the perpendicular ray which is given off along with them from the sun, and this point is the point of combustion.*

On it are collected an infinite number of rays, and the concentration of light causes combustion. But this concentration would not take place except by double refraction, as shown in the diagram.' (*Opus Majus*, Part VII.)

'*Glasses* (*perspicua*) *can be so constructed that objects at a very great distance appear to be quite close at hand, and conversely.* Thus we read the smallest letters from an incredible distance, number objects, however small, and make the stars appear as near as we wish. . . . Also objects can be made to appear so that the greatest seems the least, and conversely; what are high appear low and short, and conversely; and what is hidden appears manifest. . . .

'But among the more subtle powers of construction is this of directing and concentrating rays by means of [instruments of] different forms and reflections at any distance we wish, where whatever is subjected to them is burned. . . . But greater than any such design or purpose is that *the heavens might be portrayed in all their length and breadth on a corporeal figure moving with their diurnal motion, and this would be worth a whole kingdom to a wise man.* Let this, then, be sufficient as an example, although an infinite number of other marvels could be set forth.' (*De Secretis Operibus Artis et Naturae.*)[1]

11. *John Peckham* (died 1292), Archbishop of Canterbury, was the author of a work on optics entitled *Perspectiva communis.* His views were mainly derived from Alhazen, though perhaps partly also from Bacon. He is important here as having drawn wide attention to optical principles. He refers to the possibility of concave lenses.[2]

12. *The Invention of Spectacles.* The names of Salvino d'Amarto degli Amarti of Florence and Alessandro della Spina of Pisa (both *c.* 1300) have become associated with the special

[1] The above passages are translated from J. H. Bridges, *The Opus Majus of Roger Bacon*, Oxford, 1897, and J. S. Brewer, *Fratris Rogeri Baconi opera quaedam hactenus inedita*, London, 1859. The best account of Roger Bacon's views on Optics has been given by E. Wiedemann, *Roger Bacon und seine Verdienste um die Optik*; S. Vogl, *Roger Bacon's Lehre von der sinnlichen Spezies und vom Sehvorgange*; and J. Würschmidt, *Roger Bacon's Art der wissenschaftlichen Arbeiten, dargestellt nach seiner Schrift De speculis.* All these three papers are contained in the volume edited by A. G. Little, *Roger Bacon, Essays contributed by various writers on the occasion of the commemoration of his birth*, Oxford, 1914.

[2] Peckham's optical work exists in a number of manuscripts, and has often been printed. The first edition is dated from Milan, 1482. It has been analysed by S. Wilde in his *Geschichte der Optik*, Berlin, 1838, Teil I, p. 83. The recent work on Peckham by P. H. Spettmann, *Die Psychologie des Johannes Pecham*, Münster, 1919, contains practically nothing on optics.

application of lenses for use as spectacles. Lenses, as we have seen, were known to Roger Bacon, who suggested also their use in aiding vision. D'Amarto and Spina are said to have applied the principle thus suggested. The evidence for this statement is not entirely satisfactory. It is as follows :

*Salvino d'Amarto.* An eighteenth-century Florentine antiquary named Leopoldo del Migliore found at the Church of Santa Maria Maggiore at Florence a gravestone with an inscription which may be thus translated : *Here lies Salvino d'Amarto degli Amarti of Florence, the inventor of spectacles. May God forgive his sins. He died anno Domini 1317.*[1] The gravestone no longer exists.

*Alessandro della Spina.* He was a monk of the Dominican convent of St. Catharine at Pisa. In a chronicle of that institution, prepared in the fourteenth century and not long after his death, the latter event was placed in the year 1313. It further recorded that *Brother Alessandro della Spina, a worthy and modest man, could fashion cunningly whatsoever was shown or described to him. Spectacles (Ocularia) were first made by him and given out with a cheerful and willing heart.*[2] This chronicle is lost.

Whatever the value of these statements there is collateral evidence that spectacles in fact came into use towards the end of the thirteenth century. (*a*) In the 1729 edition of the Dictionary of the Accademia della Crusca, under the heading *Occhiali*, we read that Giordano da Rivalto of Piacenza, a monk of Pisa, in a sermon delivered on February 23, 1305, said that 'it is not twenty years since there was discovered the art of making spectacles to see better, one of the best and most necessary of arts. . . . I have myself seen and spoken to the man who first discovered and made them'. (*b*) The naturalist Francesco Redi (1626–94) published in 1678 a manuscript which was dated 1289, in which was the following passage : 'I am so weighted with years that without the glasses called *occhiali* I could neither read nor write. These have been lately invented to the convenience of poor old people whose sight is enfeebled.'[3] (*c*) Bernard de Gordon, who was physician at Montpellier from 1285–1307, makes mention of

[1] Migliore's work, which we have not seen, was published in 1684. It is mentioned by J. J. Volkmann, *Historisch-kritische Nachrichten von Italien*, Leipzig, 1770–1, i, p. 542, but in Volkmann's time the gravestone no longer existed.

[2] *Allgemeines historisches Lexikon*, Leipzig, 1709.

[3] Francesco Redi, *Lettera sopra l'invenzione degli occhiali di naso*, Florence, 1678.

spectacles as *oculus berellinus* in his *Lilium medicinae* (written 1305, first printed Lyons 1474). This is probably the first medical work containing such a reference. It is amusing to observe that he mentions them only to recommend his own eyewash which will, he says, render them useless ! [1]

The actual application of convex lenses as spectacles can thus be placed as beginning about 1280. Roger Bacon's reference enables us to place the idea on which they are based somewhat further back, say to 1260 or to 1250. It is apparent, from what has been said, that the use of spectacles spread rapidly about the turn of the thirteenth century.[2]

13. *The introduction of concave lenses* should be treated as a stage separate and distinct from the invention of ordinary convex spectacles. Although there are many mentions of myopia in ancient and mediaeval literature, and although John of Peckham seems to have known of the possibility of concave lenses, they did not come into use until a much later date than that given for convex spectacles. Little is known of the introduction of concave glasses and the earliest references are to works of art. The first of these are probably Lucas Cranach the elder's 'Adulteress before Christ' in the Pinakothek at Munich, painted about 1500, and Rafael's portrait of Pope Leo X in the Pitti Palace, painted probably in 1517. Both of these portray spectacles, the reflex of which proves them to be concave. Leo X is known to have been a myope. These pictures are, however, very exceptional cases, and concave lenses do not appear to have become generally accessible to myopes until after the middle of the sixteenth century.[3] Maurolico (1554) understood them quite well. Jacques

[1] Bernard describes his eyewash as *tantae virtutis quod decrepitum faceret legere literas minutas sine ocularibus !* For the dates of Bernard see R. R. von Töply, 'Die chronologische Reihenfolge der Schriften des Bernard Gordon', in the *Mitteilungen zur Gesch. der Medizin und Naturwissenschaften*, vi, p. 94, Leipzig, 1907, corrected by K. Sudhoff, 'Zur Schriftstellerei Bernard von Gordon', in the *Archiv für Gesch. der Medizin*, X, p. 162, Leipzig, 1916.

[2] The question of the invention of spectacles has been frequently discussed. One of the latest writers who has traversed this field is V. Rocchi, 'Appunti di storia critica del microscopio', in the *Rivista di Storia critica delle Scienze Mediche e Naturali*, January, 1913. The best general account is that of Emil Bock, *Die Brille und ihre Geschichte*, Vienna, 1903. In English there is Carl Barck's *History of Spectacles*, Chicago, 1907, taken largely from Bock, and G. H. Oliver's *History of the Invention and Discovery of Spectacles*, London, 1913.

[3] See Jakob Stilling, *Untersuchungen über die Entstehung der Kurzsichtigkeit*, Wiesbaden, 1887.

Houllier of Paris (1498–1562) was the first physician to order them, and was soon followed by Mercuriali (1530–1606) in Italy and Forestius (about 1590) in Holland.

14. *Leonardo da Vinci* (1452–1519) represents the turning-point between mediaeval and modern in the history of Optics. If his ever active mind was continually throwing out hints the value of which was not appreciated for centuries, yet no man reflected better than he the floating opinion of his time. Leonardo suggested the use of sections of glass spheres and spectacle lenses for magnification, and he made an attempt to explain the action of convex lenses—he probably knew nothing of concave lenses—in cases of presbyopia. This condition he regarded as due to loss of power to converge, and he writes thus of the helpful action of convex lenses :

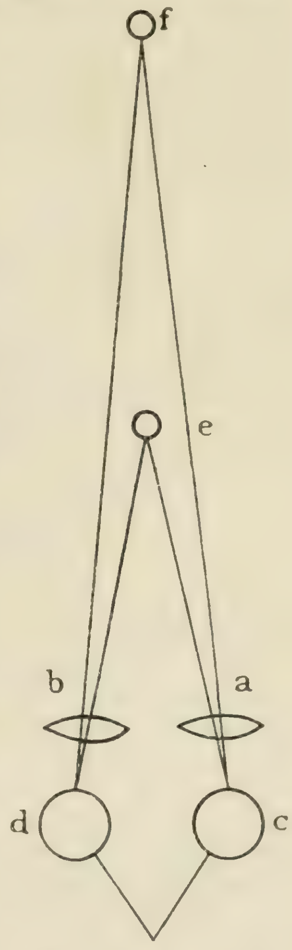



Fig. 7. THE ACTION OF SPECTACLE LENSES, according to Leonardo, redrawn from *Codice Atlantico*, fol. 244 r.

'A demonstration how lenses help vision. Let *a b* be the lenses and *c d* the eyes. When these become aged the object which they were accustomed to see easily at *e* with axes diverted from the straight line of the optic nerves [can no longer be seen so near]. For owing to the advance of age this power of turning becomes weakened so that it is not possible to converge without great pain to the eyes, and it is then necessary to move the object further off, that is from *e* to *f*, where it can be seen better but in less detail. Now, interposing the lens, the object can easily be seen at the proximity customary for youth, i.e. at *e*. This is so because the image of the object passes to the eye through a compound medium both rare and dense, *rare* as to the air between object and lens, and *dense* as to the thickness of the glass of the lens itself. Wherefore the direction turns in its course through the glass and diverts the line so that the object is seen at *e* as though it were at *f*, with the added advantage of not diverting the axis of the eye from its optic nerve, and as it is nearer it can be seen and recognized better at *e* than at *f*, especially if minute ' (Fig. 7).[1]

[1] The passage here translated is in the *Codice Atlantico* at the Ambrosian

Leonardo's solution of the action and nature of lenses is erroneous, but in this and many other passages we see him groping after a better theory of vision than was current in his day.

His description of the anatomy of the eye is very faulty though perhaps better than that of his mediaeval predecessors. He dis-

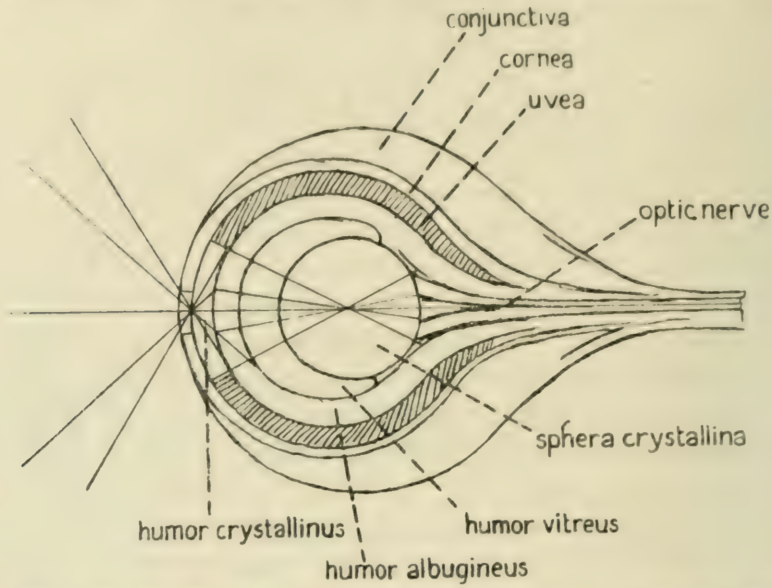



Fig. 8. THE STRUCTURE OF THE EYE, according to Leonardo. Redrawn from *Codice Atlantico*, fol. 337 r. The legends are supplied from other passages.

tinguished a *humor crystallinus*, which is really the 'lens' of modern notation distinct from the *sphera crystallina* or central

Library at Milan (published in facsimile by the Regia Accademia dei Lincei, 1899–1904). As the translation is difficult and in parts doubtful the original Italian is here given, *Codice Atlantico*, fo. 244 r.:

'Pruova come li occhiali aiutan a la vista. Siano li occhiali a b e li occhi c d quali per essere invecchiati, bisognia che l'obbietto, che soleano vedere in e con gran facilità e forte piegare il loro assis dalla rettitudine de' nerbi ottici, la qual cosa, per causa della vecchiezza, viene tal potenzia di piegare a essere indebolita, onde non si po torcere sanza gran doglia d'essi occhi, sì che per necessità son constretti a fare più remoto l'obbietto, cioè da e a f, e lì poi lo vede meglio, ma non alla minuta ; ora, interponendo l'occhiale, l'obbietto è ben conosciuto nella distanzia della gioventù, cioè in e, la qual cosa accade perchè l'obbietto e passan all'occhio per vario mezzo, cioè raro e denso, raro per l'aria ch[e è] tra l'occhiale e l'obbietto, e [den]so si è per la grossezza del vetro [delli] occhiali, onde la rettitudine [ . . . ] piega nella grossezza del vetro [e to]rce la linia a d in modo ch[e ve]dendo la cosa in e, esso la vede come in i f, per comodità di non piegare l'assis dell'occhio da' sua nervi ottici, e per vicinità la vede e conosce meglio in e che in f, e massime le cose minute.'

body of the eye (Fig. 9), but he still placed the visual function with this latter body, as did Vesalius a generation later.[1] He gives a clear figure in illustration of his view of the structure of the eye, showing also the path which he believed the rays to follow. This figure we here reproduce, adding the anatomical notation (Fig. 8). It shows the rays crossing each other twice within the globe of the eye before they reach the optic nerve. This double crossing was introduced to obviate the formation of an inverted image, the existence of which was a great stumbling-block to the Renaissance optical investigations. Leonardo stands alone in his time in showing the rays in the *humor crystallinus* pursuing a course as though through a pin-hole.

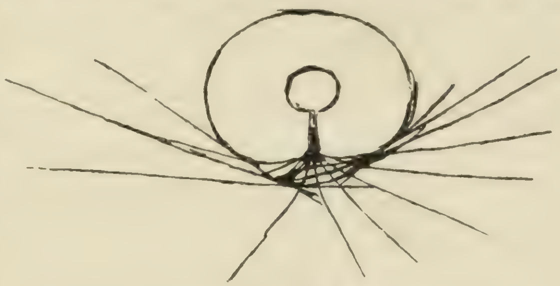


Fig. 9. DIAGRAM OF EYE, from Leonardo, showing the *sphera crystallina*, the supposed site of vision in the centre of the globe. (*Quaderni*, iv. fol. 12 v.)

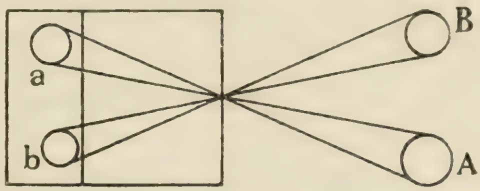



Fig. 10. THE ACTION OF A PIN-HOLE CAMERA, according to Leonardo. (*Codice Atlantico*, fol. 216 r.)

Now this conception of the action of a lens as equivalent to a pin-hole offered a fruitful line of advance. In Leonardo's own hands it led incidentally to the development of his idea of a camera obscura, the germ of which is contained in the following simple statement :

'As all lines can meet in a focus (*punto*) without interfering with each other—since they are incorporeal—so also can all *species* of objects. And as every point is within sight of the object opposite to it and every object is within sight of the point opposite to it, the diminished rays of such species can pass through this focus (*punto*) ; after passing they re-form and their size again increases but their impressions appear reversed ' (Fig. 10).[2]

[1] *Studies*, vol. i, p. 121.

[2] The Italian of this passage, quoted by J. P. Richter, *The Literary Works of Leonardo da Vinci*, London, 1883, i, § 81, is as follows : ' Siccome in un punto passan tutte le linie sanza occupatione l'una dell' altra per essere incorporee, cosi possono passarvi tutte le spetie delle superfitie, e siccome ogni dato punto rede ogni antiposto obbietto, e ogni obietto rede l'antiposto punto naturale, ancora

The word *species* here used by Leonardo is a technical term, which refers to an hypothesis of the nature of vision prevalent in the Middle Ages and derived from antiquity.[1] According to this view all visible objects are constantly giving off impalpable thin shells from their external visible surfaces. These meet with the visual spirit or emanation from the eye, and having so collided pass back together into the eye as images. Reference to this theory will be found in the description of the eye by Manfredi in the first volume of this series.[2]

What may be termed Leonardo's ' pin-hole theory ', combined with his conception of the nature of the eye and its central *sphera crystallina,* led him to suggest a practical form of camera obscura (Fig. 11), and the diagram he constructed for this apparatus is important as the earliest figure of an optical apparatus other than a spectacle lens. In this apparatus a glass globe is placed just behind the opening, and the eye placed at the focus of the globe observes the distant object. The diagram shows a double crossing of the rays as in his figure of the anatomy of the eye (Fig. 8).

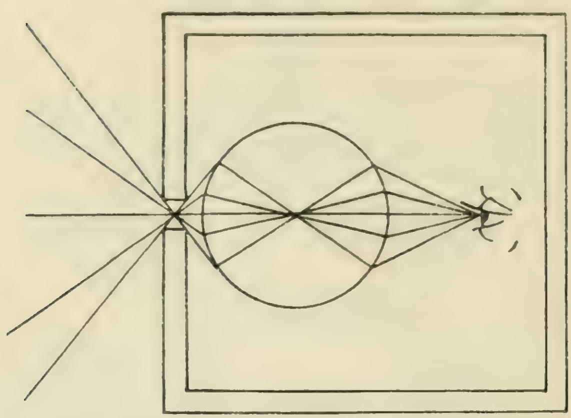


FIG. 11. A CAMERA OBSCURA, according to Leonardo. Redrawn from *Codice Atlantico*, fol. 337 r.

Leonardo's contributions, though original and valuable, remained inaccessible for nearly four centuries. It does not, however, necessarily follow, as has been too often assumed, that they were without influence, and it is at least possible that too much stress has been laid on Leonardo's intellectual isolation. Man of genius though he was, he was also a child of his age and he represents, though with the instinct of genius, the ideas of his age.[3] Thus we

per esso punto possono transire i diminute razzi di tali spetie, dopo il transita delle quali si riformeranno e ricresceranno le quantita di tali spetie.'

[1] It is set forth, for instance, in the *Timaeus* of Plato, which was very influential in the Middle Ages through the early Latin translation of Chalcidius.

[2] *Studies*, vol. i, p. 118.

[3] Leonardo left his scientific remains in a state of confusion, and they have

find a generation or two later his views on the camera obscura developed by Digges, and his 'pin-hole theory' and view of the double inversion of images in the eye given a mathematical direction by Maurolico.

15. *Girolamo Fracastoro* (1478 ?–1553) was a suggestive writer, who devoted considerable space to a rather confused account of refraction. In the course of this discussion he has the following passage:

> '[Not only the character but] also the position of the medium affects the appearance of the objects seen, as may be observed with spectacle lenses (*in specillis ocularibus*). For if the lens be placed midway between eye and object, it appears much larger than if the lens is made to approach the object or the eye.[1] . . . Glasses (*specilla ocularia*) may be arranged of such density that if any one looks through them at the moon or at any star they appear near and hardly higher than the steeples.[2]

It is possible that he was here contemplating a bilenticular apparatus.[3]

16. *Francesco Maurolico* (1494–1577) made perhaps the first published independent attempt, after Bacon and Peckham, at a mathematical analysis of the optics of the lens and of the eye. He is thus the predecessor of Kepler. There was another important service that Maurolico rendered to optical science. He dethroned the lens (*humor crystallinus*) from its pride of place, occupied ever since Greek times, as the seat of vision. There were, however, two great errors from which he could not free himself. He believed, firstly, that every point of an object seen must correspond to only a single visual ray, and secondly, that the image in the eye is not reversed. His view, however, that short sight was due to excessive and far sight to defective power of the crystalline lens, though erroneous, was yet a fruitful and in its day not untenable hypothesis.[4]

17. *Leonard Digges* (died 1571 ?) was the first to whom can be definitely attributed the construction of a bilenticular system.

suffered much by time, misuse, and neglect. It is impossible to give a bibliography here, but his optical results are summarized by E. Solmi, 'Leonardo da Vinci e il metodo sperimentale nelle ricerche fisiche', in the *Atti e memorie della R. Accademia Virgiliana di Mantova*, Mantua, 1905, and by O. Werner, *Zur Physik Leonardo da Vincis*, Berlin, 1911.

[1] *Homocentrica*, ii. 8. [2] *Homocentrica*, iii. 23.

[3] The *Homocentrica* in which these passages occur was first printed at Venice in 1538. The scientific value of this work is discussed by the present writer in an article in the *Annals of Medical History*, vol. i, p. 1, New York, 1917.

[4] Maurolico's work, *Photismi de lumine et umbra*, was printed in Venice in 1575.

The evidence for this statement rests on the following passage in a work printed in 1571 by his son, Thomas Digges (died 1595):

'Marueylouse are the conclusions that may be perfourmed by glasses concaue and conuex of circulare and parabolicall fourmes, using for multiplication of beames sometime the ayde of glasses transparent, which by fraction should unite or dissipate the images or figures presented by the reflection of other. By these kinds of glasses or rather frames of them, placed in due angles, ye may not only set out the proportion of an whole region, you represent before your eye the lively image of euery towne, village, etc., and that in as little or great a space or place as ye will prescribe, but also augment and dilate any parcell thereof, so that whereas at the firste apparance an whole towne shall present it selfe so small and compacte together that ye shall not discerne any difference of streates, ye may by applycation of glasses in due proportion cause any peculiare house or roume thereof dilate, and shew it selfe in as ample fourme as the whole towne first appeared, so that ye shall discerne and trifle or reade any letter lying there open, especially if the sonne beames may come un to it, as playnly as if you wer corporally present, althouh it be distante from you as farre as eye can discrye. But of these conclusions I minde not here more to intreate, hauing at large in a volume by it selfe opened the miraculous effectes of perspective glasses.'

Digges's system appears to have been combined in some manner with a camera obscura. Unfortunately, his further description of it was never published.[1]

In a second edition of the same work, printed in 1591, Digges tells us that:

'My father, by his continuell painfull practices, assisted by demonstrations mathematical, was able, and sundrie times hath, by proportional glasses, duly situate in convenient angles, not only discovered things farre off, read letters, numbered pieces of money, with the very coyne and superscription thereof, cast by some of his freends of purpose upon downs in the open fields, but also seuen miles off declared what hath been done in priuate places.'

18. *Felix Plater* (1536–1614) made an important addition to the optical theory of the eye, and rid himself of at least one of Maurolico's fundamental errors. In a work printed in 1583 he clearly ascribes to the crystalline lens and to the retina the functions with which we are accustomed to credit them. 'The *humor*

[1] The work of Thomas Digges in which this passage occurs is entitled *A Geometrical Practise named Pantometria*, and was printed in London in 1571.

*crystallinus* ', he says, ' is the lens of the nerve of vision (*perspicillum nervi visorii*) and, placed in front of it and of the foramen of the pupil, it collects as rays the *species* impinging on the eye, and sends them forth magnified to the circuit of the retina so that it may the more easily appreciate them. . . . The primary organ (*pars primaria*) of vision, the optic nerve, on reaching the eye expands into a hollow retiform hemisphere. This receives and distinguishes the *species* and colours of external objects by the light which, passing into the eye through the foramen of the pupil, is distributed (*representata*) by its lens as has been said above.'[1]

19. *Giambattista della Porta* (1540–1615) is the first to whom can be attributed the actual combination of lenses in the form of a microscope. This statement rests on the evidence of the following passage in his *Magia naturalis*: ' Concave lenses enable one to see far off more clearly, while convex ones make near objects more discernible.' He was thus apparently myopic, and he goes on to say that ' with a concave lens things afar appear smaller but plainer, with a convex lens you see them larger but less distinct. If, however, you know how to combine the two sorts properly, you will see near and far both large and clear.' In later years, when the microscope became a recognized instrument, much larger claims were made by and for Porta, but there is no real evidence that he made any effective practical application of his idea.[2]

But Porta made a more valuable contribution to optics than his accidental suggestion of the possibility of a microscope and telescope which he did not follow up, for his is the first printed book containing a clear comparison of the action of a lens to that of a pinhole camera. By this suggestion he made possible the mathematical developments of Kepler. ' Just as objects illumined by the sun ', says Porta, ' are set forth on a sheet of paper intercepting the beam from a small hole in a shutter, so does the light passing through the foramen papilla form images of the objects seen by the action of the crystalline lens (*subiens in crystallinum*).'

20. *Hieronymo Fabrizio* of Aquapendente (1537–1619), the teacher of Harvey, is worthy of a brief mention here as following up the optical work of Maurolico in the field of anatomy. This admirable and accurate observer in his *Tractatus de oculo visusque organo* (Padua, 1601) was the first to figure an eye with a definite

[1] Felix Plater, *De partibus corporis humani structura et usu*, Basel, 1583, p. 187.

[2] Porta's *Magia naturalis* was first printed at Naples in 1558, but the passages in question do not occur in it, nor in any edition until that of 1588.

lens placed in the front part of the globe. His conservative attitude, however, prevented him from accepting the views of Plater and he still placed the seat of vision in the lens. It is a pity that a somewhat cumbrous learning and an undue respect for authority marred the value of this as of much of the other work of the great Paduan professor.

21. *The Empirical Discovery of the Telescope and Compound Microscope.* There are at least three claimants, all Dutchmen, that have to be seriously considered: Zacharias called Jansen, Jan Lippershey, and James Metius otherwise known as Jacob Adrianzoon. On whichever our choice may fall, however, the date of the discovery may be placed between 1591 and 1608, and probably nearer the later than the earlier date. The reader may be reminded that at this period lenses both concave and convex were well known and habitually used for spectacles. The profession of 'spectacle maker' had become quite recognized and lucrative although little scientific attention had as yet been directed to the nature and application of lenses. The three claimants may be considered separately.

(*a*) *Zacharias*, son of Jan, and known as Jansen (1580–16 ?) of Middelburg, is usually regarded as the first who actually constructed a microscope. His first attempt was the result of an accident. It appears that while still a lad and at work in the shop of his father, who was a spectacle maker, he happened to place two lenses in a tube and found that they acted as a microscope or telescope. Effective instruments were constructed by him in the first decade of the seventeenth century. The evidence that Jansen was really the first constructor of these bilenticular instruments rests on the testimony of Willem Boreel (1591–1668), the Dutch Ambassador to France. Boreel's evidence is given in a letter by him to Pierre Borel (1620–71), which runs as follows:

'I am a native of Middelburg, the capital of Zeeland, and close to the house where I was born . . . there lived in the year 1591 a certain spectacle maker, Hans by name. His wife, Maria, had a son Zacharias, whom I knew very well, because, as a neighbour and from a tender age I constantly went in and out playing with him. This Hans, or Johannes, with his son Zacharias, as I have often heard, were the first to invent microscopes, which they presented to Prince Maurice, the governor and supreme commander of the united Dutch forces, and were rewarded with some honorarium. Similarly, they afterwards offered a microscope

to the Austrian Archduke Albert, supreme governor of Holland. When I was Ambassador to England in the year 1619, the Dutchman Cornelius Drebbel of Alkomar, a man familiar with many secrets of nature, who was serving there as mathematician to King James, and was well known to me, showed me that very instrument which the Archduke had presented as a gift to Drebbel, namely, the microscope of Zacharias himself. Nor was it (as they are now seen) with a short tube, but nearly two and a half feet long, and the tube was of gilded brass, two fingers breadth in diameter, and supported on three dolphins formed also of brass. At its base was an ebony disk, containing shreds or some minute objects which we inspected from above, and their forms were so magnified as to seem almost miraculous.' [1] His instruments were probably made of two convex lenses.

(*b*) *Jan Lippershey* of Wesel (flourished 1608). It is evident that at or about the period 1608–10 there were other Dutch mechanics on the track of the discovery of Zacharias, for Borel goes on to say that

'long after, in 1610, after much research, these individuals (i. e. Zacharias and his father) invented, in Middelburg, the long sidereal telescope with which we gaze at the moon and planets and presented one of them to Prince Maurice, who deemed it prudent to conceal the invention in order to make use of it in military operations. While rumours were abroad regarding the invention, a stranger came to Middelburg, and having asked for a spectacle maker, was shown by mistake into Lippershey's shop. From the questions put by the stranger, Lippershey, being a shrewd man, was able to detect the construction of the instrument, and having succeeded by this means in making telescopes, he was generally considered the real inventor of them. However, the mistake was soon discovered, for Drebbel, upon his return to Holland, proceeded with Adrian Metius to Jansen's shop, and purchased telescopes of him.'

This account treats Lippershey as a mere imitator of Zacharias. There is however evidence, though not of an entirely convincing character, that the discovery was really made independently by Lippershey. When Borel investigated the matter about 1645 he examined several witnesses, three of whom attributed the discovery of telescopes to Lippershey, one of them placing it before 1605 and the other two in 1609 or 1610. Moreover

[1] This passage is contained in a work by Pierre Borel, *De vero telescopii inventore cum brevi omnium conspiciliorum historia*, The Hague, 1655. Borel produces other witnesses for Zacharias; one of them, his son, gives the year as 1590; the other, his sister, brings it down ambiguously to 1611 or 1619. Neither is worthy of prolonged consideration.

Antoine-Marie Schyrle de Rheita (1597–1660), a competent mathematician and acquaintance of Kepler, writing in 1645, tells also that Lippershey invented the telescope in 1609. Lippershey, according to him, happened to place a concave before a convex lens and found that the weathercock of a neighbouring church appeared larger and nearer. He fitted the glasses in a tube and amused his customers with this instrument. The Marquis of Spinola, who was then at the Hague, bought the instrument and presented it, according to de Rheita, to the Archduke Albert of Austria by whom the invention was made known. The whole story is suspiciously like that of Zacharias Jansen, and the two are perhaps a confusion of the same original account.[1]

More satisfactory evidence in Lippershey's favour has been obtained from the Acts of the States-General preserved in the Government archives at the Hague.

'On the 2nd of October, 1608, the Assembly of the States took into consideration the petition of John Lippershey, spectacle maker, a native of Wesel, and an inhabitant of Middelburg, inventor of an instrument for seeing at a distance. A committee was appointed to confer with Lippershey, for the purpose of ascertaining whether it might not be possible to improve the instrument so as to enable one *to look through it with both eyes*. On the 4th of the same month it was resolved that certain of the members should test the instrument of Lippershey, by observing with it from the turret of Prince Maurice's mansion. It was further resolved, that if the perspective should be found useful, an engagement should be entered into with the inventor, to execute three such instruments of rock crystal, and that he should be enjoined not to divulge the invention to anybody. On the 6th of the same month, the Assembly agreed to give Lippershey 900 florins for such an instrument. On the 15th of December they examined the instrument invented by Lippershey *to see with both eyes*, and approved of it; but as many others had a knowledge of this new invention to see at a distance, they did not deem it expedient to grant him an exclusive privilege to execute such instruments. However, they gave him orders to execute, for the use of the Government, two other instruments to see with both eyes, allowing him the same remuneration for his services as in the first instance.'[2] The

[1] A.-M. Schyrle de Rheita, *Oculus Enoch et Eliae, sive Radius sidereo-mysticus*, Antwerp, 1645.

[2] The application and findings of the committee that sat on Lippershey's petition were published by G. Moll, 'On the first invention of telescopes collected from the notes and papers of the late Professor van Swinden,' *Journal of the Royal Institution*, vol. i, pp. 319 and 483, London, 1831; see also R. Grant, *History of Physical Astronomy*, p. 521, London, 1852.

instruments of Lippershey were almost certainly made of a convex and concave lens and therefore did not invert.

(*c*) The claim of *James Metius* (Jacob Adrianzoon, died *circa* 1629) may now be set forth, and a consideration of it enables us to understand why the States-General did not fully accede to the petition of Lippershey. There is documentary evidence that while the claim of Lippershey was still under consideration by the States-General, viz. on October 17, 1608, James Metius of Alkmaar put in a petition to obtain the exclusive right to sell an instrument of his invention for making distant bodies appear larger and more distinct, and that at the time of his petition he knew of the instrument 'that had lately been offered to the States *by a citizen and spectacle maker of Middelburg*', i. e. presumably by Lippershey. This petition survives in the correspondence of Christiaan Huygens (1629–95) and has recently been published.[1]

Of James Metius we can form a rather better picture than of the other two claimants. He was the son of Adrian Metius (Adrianzoon or Anthonisz, 1527–1607) who became Burgomaster of Alkmaar in 1573. Adrian, who was born in Metz and hence got his name *Metius*, has a place in the history of pure Mathematics for his solution of $\pi$ as equal to $\frac{355}{113} = 3{\cdot}1415929\ldots$[2] This Adrian Metius had two sons, Adrian and James. Adrian Metius (1571–1635) was himself a mathematician and was doubtless the man of that name who is referred to by Borel as purchasing a telescope along with Drebbel at Zacharias Jansen's shop in Middelburg. The other son, James, was less gifted. Of him Descartes wrote:

'It is about 30 years since one named Jacques Metius, a man who had never studied (but who had a father and brother that followed mathematics as a profession), though he himself loved to make mirrors and burning-glasses, . . . having by him glasses of various shape, had the good luck to look through two of them, of which one was a little thicker in the centre than at the edge and the other was much thicker at the edges than at the middle; and he luckily put them in the ends of a tube, and thus the first telescopes were made. It is on this model only that all the others which have been seen since have been made, without any one yet,

[1] In the *Œuvres complètes de Christiaan Huygens*, vol. xiii, fasc. 1, p. 591, The Hague, 1916. Huygen's view of the history of the invention, with which our own conclusions are in accord, is given on p. 436 of the same volume. The passage is translated from the Dutch in the appendix on page 533.

[2] Cp. Moritz Cantor, *Vorlesungen über Geschichte der Mathematik*, 4 vols., vol. ii, 2nd edition, Leipzig, 1900, p. 599.

so far as I know, having adequately determined the shapes that the glasses ought to have.'[1]

There are conflicting elements in these accounts, but from them can be drawn the following deductions which seem to sum up the probabilities.

i. The invention of the microscope probably preceded that of the telescope.

ii. The invention of the microscope was the work of Zacharias Jansen after 1591 and before 1608. It was perhaps formed of two convex lenses.

iii. The invention of the microscope by Zacharias Jansen was followed by that of the telescope about 1608 by Lippershey and Metius. It was the military applicability of the apparatus that drew attention to it.

iv. This first telescope was of the Galilean type with a concave eye-piece and a convex objective.

22. *Galileo* (1564–1642) was the *effective* discoverer of both telescope and microscope. The event has often been described and may be referred to the early part of 1609. The story, in his own words, is briefly thus:

'About ten months ago', he says, 'a rumour reached me of an ocular instrument made by a certain Dutchman by means of which an object could be made to appear distinct and near to an eye that looked through it, although it was really far away. . . . And so I considered the desirability of investigating the method, and I reflected on the means by which I might come to the invention of a similar instrument. A little later, making use of the doctrine of refractions, I first prepared a leaden tube, at the ends of which were placed two lenses, each of them flat on one side, and as to the other side I fashioned one concave and the other convex. Then, moving the eye to the concave one, I saw the objects fairly large and nearer, for they appeared three times nearer and nine times larger than when they were observed by the naked eye. Soon after I made another more exactly, representing objects more than sixty times larger. At length, sparing no labour and no expense, I got to the point that I could construct an excellent instrument so that things seen through it appeared almost a thousand times greater and more than thirty-fold nearer than if observed by the naked eye.'[2]

In another work he says:

'Some would tell me that it is of no little help in the discovery

[1] The passage will be found in the *Dioptrique*, and can be referred to in the *Œuvres de Descartes* published by C. Adam and P. Tannery, vol. vi, Paris, 1902, p. 82, and note on p. 227.

[2] *Siderius Nuncius*, Venice, 1610.

and resolution of a problem to be first of all in some way aware of the true conclusion and certain of not being in search of the impossible, and that therefore the knowledge and the certainty that the microscope had indeed been invented had been of such help to me that perchance without that I should not have discovered it. To this I reply that the help rendered me by the knowledge did indeed stimulate me to apply myself to the notion, and it may be that without this I should never have thought of it. Beyond this I do not believe that knowledge to have facilitated the invention. But, after all, the solution of a problem, thought out and defined, is a work of some skill, and we are not certain that the Dutchman, the first inventor of the telescope, was not a simple maker of ordinary lenses who, casually arranging glasses of various sorts, happened to look through the combination of a convex and a concave one placed at various distances from the eye and in this way observed the effect that followed thereon. But I, moved by the knowledge given, discovered it by a process of reasoning.' [1]

23. The telescope used by Galileo had of course a concave eyepiece and a convex objective. Specimens of the telescopes actually used by him—though not of the earliest type—are still in existence and are on exhibition at the Galileo Museum in Florence. Galileo, in spite of his claim to have invented the telescope on a theoretical basis, did not really arrive at a successful solution of the optics of the lens, and his account of the path of light in the bilenticular system is unsatisfactory. It was improved by *Kepler* (1571–1630) in his *Dioptrice* (Cologne, 1611), and he at the same time suggested that form of telescope and of microscope, consisting of two convex lenses, which has developed as our modern instrument. Kepler had already some years earlier made a mathematical analysis of the properties of a simple lens in his *Ad Vitellionem paralipomena, quibus astronomiae pars optica traditur*, Frankfort, 1604, and his optical labours were now crowned by his description of the bilenticular system. It should be added that in the year of the appearance of Kepler's *Dioptrice* (1611) appeared the work of Marco Antonio de Dominis (1566–1624), *De Radiis Visus et Lucis in Vitris Perspectivis et Iride*, containing suggestions somewhat similar to those of Kepler. In view of the well-known dishonesty of the notorious Archbishop of Spalatro, it is difficult to determine to what extent these ideas were his own. In any event the priority of effective demonstration rests with Galileo, and of the publication of mathematical analysis with Kepler.

SEE APPENDIX, PAGE 533.

[1] *Il Saggiatore*, Rome, 1623.

# HYPOTHESIS

By F. C. S. Schiller.

§ 1. Deep down in the heart of man lives a chronic *Fear* of the weird world in which he finds himself so inexplicably plunged and is so imperiously prompted, by hereditary instinct—and the fear of greater evils—to struggle for his existence to the last breath. This Fear is usually kept under by the fabricated order of the social life, and kept drugged by the traditions, conventions, creeds, and mechanisms of a social *routine*. But under abnormal conditions it breaks out, and renders him a prey to wild and senseless 'superstitions'; which are in truth survivals from the times when as yet no 'science' had been devised to reassure men by its claim to curb the chaos of happenings, superstitions which still betray our unavowed distrust of the exorcisms of the verbal formulas which we significantly call the *laws* of nature—in the *hope* that nature will *obey* them. This Fear moreover begets not only the superstitions but the gods: we may expect to find traces of its universal influence, *camouflaged* of course, disseminated also through the doctrines of the sciences, and revealing itself as an obstinate bias or persistent prejudice against such endeavours as run counter to it.

For it must not be supposed, of course, that Fear is the only attitude which the world inspires in man. Fear alone would be utterly inadequate to keep us alive. It must be matched with Courage—even if only that of desperation—so as to generate a prudent circumspection and to lead to successful action. Accordingly, in his dealings with nature man cannot always remain cowering on the defensive. He must *react*, must show initiative, must attack the problems of his life, must *experiment* boldly, and manipulate and alter the given conditions, 'speculating' and running risks. And, if he is defeated, as he always is at first, he must never say die, but must try again, more intelligently.

§ 2. The interplay between these antagonistic and yet complementary attitudes may be traced through all life, and we may expect to find it operative also in the science of Logic. We shall

further find that these attitudes appeal differently to the different schools of logic. To an *intellectualistic* logic it will seem self-evident that sound thought must play for safety and endeavour to proceed by *valid* steps to an *assured* conclusion. It will eschew therefore or minimize, so far as may be, risks and hazardous guess-work ; it will admire and revere formulas and forms, which, it will fondly believe, cannot go wrong, but assure success, like spells, to those who have mastered them. Hence the perennial charm of formulas like a ' syllogism ' or a ' universal ' and their prominence in logics of this kind. They function as ' words of power ' ; to pronounce them is deemed enough to flood with light the obscurest problem, to rehearse them, like magic charms, seems essential and sufficient for salvation.

To a *voluntaristic* logic this attitude seems merely superstitious. It demands not words but deeds. So it will value a formula strictly according to its achievements in actual use. It will not labour, vainly, to construct a fool-proof form, nor imagine that by clinging to it faithfully any one can guard himself against any error. It will perceive that the value, the truth, the meaning, the difficulty, and the danger, of every form lie in the application thereof, and that to risk the application of a principle, is to run a risk of error. For every principle may be misapplied to a case it does not really fit. But seeing that this risk in real reasoning is as unescapable as death, this bolder logic will not shrink from it. It will recognize and sanction the running of risks, provided that they are taken open-eyed and not blindly, and the desire for logical *value* is not frustrated by being exalted into a demand for absolute *validity*. It will approve of operations expressive of spontaneous, and still more of conscious, experimentation, of intelligent anticipation, of the mental attitude of unending inquiry, of an insatiate appetite for new truth and more knowledge.

In short, it will recognize that *knowing is an adventure*, in which to progress we must not be content to take our stand always on firm ground, whether of ' solid fact ' or ' fundamental principle ', but must learn to *swim* the breakers of Doubt, and win our way through the ocean of Becoming to the Happy Isles in that immeasurably distant West, where, if anywhere, those weary of activity can rest in eternal bliss, because every problem has been solved and every aim attained.

§ 3. In no department of logical doctrine do the contrast and

clash of these two tendencies in logic come out more copiously and instructively than in the treatment of Hypothesis. For this is just where the need for cognitive enterprise must impress itself even upon the most reluctant minds. Accordingly Hypothesis receives a somewhat Cinderella-like treatment in intellectualist logics. It is disparaged and intellectualized as much as the facts will allow, though, since Bacon's egregious failure, no one has dared to prohibit it altogether. But a meagre chapter is the most they can endure to waste upon it. Even out of that they are careful to expunge the real essence and significance of the mental attitude which dares to tackle nature with hypotheses and endeavours to enmesh the monsters of the deep in the flimsy fabrics of man's imagining. The scope and value of hypotheses are studiously minimized, and their use is restricted by regulations which purport to curb their licence and to ensure their soundness. But the simple truth is that intellectualist logicians are *afraid* of them.

The matter can best be elucidated by a few illustrations which will show how valiantly, and yet how vainly, logicians struggle against conceding the right of experimenting with hypotheses, and refuse to regard it as chief among the *intellectual* Rights of Man.

§ 4. We may consider first the most modern of the large 'orthodox' *Logics*, that of Professor Bosanquet. When we look up its definition of Hypothesis[1] we find that it is called "any conception by which the mind establishes relations between data of testimony, of perception or of sense, so long as that conception is one among alternative possibilities, and is not referred to reality as a fact".

There are several points to note in this definition.

(1) The essence of Hypothesis is said to be a *conception,* i.e. a mental *content,* not a mental *attitude*—the attitude to wit of *inventing* a mental content, of *holding it in suspense,* of taking it as a possibility not as an assured fact, of assuming it experimentally for the argument's sake and in order to test its value by its consequences.[2]

[1] Vol. ii, ch. v, *init.*

[2] Although his definition had thus subsumed 'hypothesis' under the notion of 'conception', Professor Bosanquet nevertheless subsequently has occasion to reverse this relation, and to say (p. 160) that (certain) "conceptions may be regarded as hypotheses in course of development and proof". This would seem

(2) The stress plainly falls on the 'conception' and the 'relations' it establishes rather than on the mind's activity and intention in forming its conceptions and establishing the relations by applying them. Yet the mental attitude of hypothesis-building is evidently the motive force in the whole process: it is this which selects and uses the conceptions which seem likely to solve the problems which engross the mind. This is particularly plain in cases where the conceptions used are vague, inchoate, dubious and plastic, and in fact *most* 'hypothetical': for we can then feel how masterfully the hypothetical attitude *moulds* our conceptions as we employ them on our data and develop them to suit its purposes.

(3) Professor Bosanquet's definition retains the traditional, but unworkable, antithesis between 'hypothesis' and 'fact'. This however is nothing but an obsolete dogma which has come down from the naïve days when logicians believed that 'facts' were solid and immutable and the firm 'foundations' of the sciences. It has unfortunately survived unceasing demonstrations in the history of every science that its 'facts' are relative to its 'theories' and 'principles', that anything may be *feigned*, if it is not found, and taken as fact *hypothetically*, and that there is no hard and fast line to be drawn between hypothesis and fact, because the conclusions of hypothesis consolidate into 'facts' when they are sufficiently confirmed, and may then themselves form the basis for further hypotheses. The difference between fact and fiction is not that in the former the mind is somehow coming in contact with an extra-mental independent real, while in the latter the *intellectus sibi permissus* runs riot and is creating freely out of nothing; we are in *both* cases manipulating mental contents, not, that is, 'the real' facts, but facts-as-they-seem-to-us in the apprehension of the imperfect knowledge of a limited intelligence—nor yet *mere* fictions, but fictions-suggested-by-the-real we *suppose* ourselves to be encountering, and *believed* to be applicable and relevant to that. When such a scientific fiction [1]

to admit that *some* conceptions are essentially hypothetical instruments for exploring nature withal; and he even says (p. 159) that "in an ultimate sense there is no knowledge without Hypothesis". Why then not adopt this ultimate sense? Would not a little more daring declare *all* knowledge to be the great hypothesis by which man, alone of living beings, endeavours to control his world?

[1] I am not here using the term in its strict technical sense as an assumption, *known* to be false, but also methodologically *convenient*.

turns out to be *very* relevant, what more natural and proper than that it should be raised to the rank of 'fact'? The history of scientific conceptions is full of illustrations, though none, perhaps as instructive and romantic in its vicissitudes as the history of the 'atom', a metaphysical invention in its initial stage, long rejected by the best scientific opinion as unprofitable, then revived and turned into a good working-hypothesis of chemistry, criticized anon as unverifiable and barely holding its ground, as a convenient fiction against its 'energetic' critics; until finally by a dramatic revolution the hypothesis of atomic dissociation explained the facts of radio-activity, and incidentally, turned the atom into a fact in the very act of exploding it and depriving it of the ultimateness and indivisibility its very name had implied for ages. But the lesson of this history will not have been truly learnt until it has inculcated into men of science a proper indifference towards the ontological pretensions of the atom's hypothetical successors, the 'electrons' and 'quanta', and they are content to accept them as the smallest coins in the reckonings of science, because, and so long as, they serve their purposes and perform this function.

§ 5. J. S. Mill's account of Hypothesis [1] is not at any rate open to the charge of excluding from its definition the essential attitude of *supposing*. It declares that "an hypothesis is any supposition which we make (either without actual evidence, or on evidence avowedly insufficient) in order to endeavour to deduce from it conclusions in accordance with facts which are known to be real; under the idea that if the conclusions to which the hypothesis leads are known truths, the hypothesis itself either must be, or at least is likely to be true".

This however is not so much to define Hypothesis itself, as a mental attitude and product of mental activity, as to define its cognitive function, i. e. what he conceives to be the proper *use* of Hypothesis in scientific inquiry. This is why he equips his definition with a number of restrictive regulations. Had he merely wanted to describe the procedure itself, he would have *begun* by pointing out that "an hypothesis being a mere supposition, there are no other limits to hypotheses than those of the human imagination", and then have gone on to investigate how the human

[1] *System of Logic*, Book iii, ch. 14, § 4.

imagination is actually limited in its guesses. This would probably have convinced him that its play was both limited by, and relative to, its experience of reality, and that it was not therefore urgently necessary to make the regulation of this activity too strict.

Interpreting in accordance with his general theory of logic, Mill goes on to remark that " hypotheses are invented to enable the Deductive Method to be earlier applied to phenomena ". This they do by taking the place of the first " indispensable " (*sic !*) step in the Method, viz. ' induction ', so that " the law is not acquired by direct induction from the facts but assumed as a hypothesis ", after which ' ratiocination ' and ' verification ' proceed as before. He contends, however, that in some cases the verification attains to the ' validity ' of an experiment according to the canon of the Method of Difference, and that then the hypothetical ' law ' is fully proved. But the assurance given by this Method " cannot be obtained when the cause assumed by the hypothesis is an unknown cause imagined solely to account for the observed effects." In the discovery of the law of gravitation, " if it had not been previously known that the planets were hindered from moving in straight lines by some force tending towards the interior of their orbit ; though the exact direction was doubtful ; or if it had not been known that the force increased in some proportion or other as the distance diminished, and diminished as the distance increased, Newton's argument would not have proved his conclusion." " The most genuinely scientific hypothesis ", therefore, must not be " destined always to remain an hypothesis, but be of such a nature as to be either proved or disproved by comparison with observed facts." In such cases alone " verification is proof " (*ib.*, p. 15), and this was the meaning of Newton's maxim that the agency alleged by a hypothesis must be a *vera causa*.

Now in this account there is, amid much sound sense, an admixture of error, which is traceable to the desire to force scientific procedure into conformity with the ideal of logical proof.

(1) If Hypothesis can take the place of Induction in the ' Deductive Method ' and a verified hypothesis can become a fact, with the highly desirable result of accelerating scientific procedure, is not the bottom knocked out of Mill's whole conception of inductive science ? His ' facts ' are no longer necessary, seeing that they may with advantage be replaced by hypotheses ; nor

are they absolute facts, but ‘ facts ’ accepted in virtue of their relation to verified hypotheses. It does not really matter whether the *data* from which we reason are wholly hypothetical, or believed to be ‘ facts ’, or something betwixt and between that is alleged but disputed ; nor is there any need to quarrel about their precise status. It is more advantageous and expeditious to go on investigating. For if we are willing and able to verify our conclusions, we shall presently become aware of the value of our *data*, which does not depend on their initial claims, but on the results they lead to.

(2) The notion that a hypothesis can only be proved by disproving all alternatives and showing that no other can satisfy the *data*, must surely be dismissed as a chimera. The only alternatives that can be either proved or disproved are those which have suggested themselves to human ingenuity, and there is no reason to think that this has, at any time, exhausted the possibilities, or to presume that it will ever do so. Moreover, the test proposed by Mill, viz. the conformity with fact of the deductions from the true hypothesis and of these *alone*, is essentially illusory.

For the reason, *inter alia*, that such conformity can never be shown to be *exact*. No sense-organ and no instrument can ever measure exactly, no observation can wholly eliminate the errors of observation, no observation can be repeated absolutely, and no two observers will absolutely coincide in their estimates. Hence, however closely the observations approximate to the deductions from the hypothetical ‘ law ’ intended to explain them —and any such approximation is likely to be exaggerated by the influence of the hypothesis on the observer—it can never be argued that they conform with *this* law *and no other*. It is always possible that some other law, or combination of laws, would fit the observations as closely as, or more closely than, that accepted. The alternatives might often be less *simple* ; but simplicity is an appeal to man’s convenience, not nature’s. The simplicity of the ‘ laws ’ first tried is methodological ; it is right to use the simplest hypothesis, if it suggests itself. Moreover, the simplicity of our first approximations is often deceptive ; our laws of nature are *statistical* ; they formulate the *average* only of what may turn out to be on closer inspection seriously divergent activities, and it is well known that statistics smooth out individual differences. Moreover, curiously enough, the progress of science has, since

Newton's day, quite familiarized physicists with the idea that even the law of gravitation may not be as exactly true as was supposed.[1] It may only express, roughly, the motions of large masses, without applying to ' electrons ' ; and even astronomers are now led to speculate whether the observed fact of the much more rapid motions of the ' older ' types of star should not be taken to mean that gravitation is not an initial endowment, but an *acquired habit*, of celestial bodies.

Lastly, it should be remembered that the alternative hypotheses, which seem to be equally capable of interpreting the facts, are often worlds apart in their character. It hardly matters to the calculations of physics whether bodies are taken to be solids moving through an ' ether ' that cannot be shown to impede their motions, or areas of diminished density in an adamantine ether. It hardly matters to biology whether the adaptations of organisms be regarded as teleological or as pseudo-teleological. In ethics, determinism + a little ignorance and habit + a little indetermination will provide interpretations of human actions that seem equally complete. The political philosopher, though not the party man, will recognize that the conservative and the innovator will always be able to make out a case for the diametrically opposed views they take of social changes. And so forth and so on. Alternative hypotheses therefore must always be admitted to be possible in principle, even where there are none actually contending for acceptance.

It follows that Verification never can be proof, if ' proof ' be taken to involve a claim to *absolute* truth. No verification will ever prove more than that the observations are *compatible* with the hypothesis they test. That does not prevent them from being equally compatible with other hypotheses, if these lead to very similar deductions, and if the observations are not exact (as we have seen they never are), or have been vitiated by the influence of a dominant hypothesis. Should a philosopher be disposed to be too sanguine about the capacity of ' facts ' to impose themselves on prejudice and to resist misinterpretation, he should be invited to consider how overwhelming were the ' proofs ' of witchcraft until about two hundred years ago.

[1] Of course had this been written in 1919 instead of 1918, one could not but have referred here to the triumphant verifications of Einstein's theory of relativity and the consequent superseding of Newton's law of gravitation.

(3) Newton's doctrine about a *vera causa* should no longer be allowed to impede the freedom of scientific movement. If it is intended as an *ex post facto* description of a hypothesis which *has* worked satisfactorily, it is the merest truism. For *after* we have solved our problem we can of course always say *which* of the alleged 'causes' was a 'true cause'. If, on the other hand, it is meant as a rule of research, it is futile and false. For then *ex hypothesi* we have not yet found a true cause, but are still looking for one. If the maxim means that no causes are to be tried but such as are similar or analogous to causes already accepted, it does indeed chime in well with man's instinctive conservatism, and may claim perhaps a certain methodological merit as proclaiming that it is well to try familiar agencies before resorting to the unknown, somewhat superfluously, because this is what both imagination and indolence will prompt us to do; but how does this pledge *nature* to submit to analysis by 'causes' which are familiar enough to seem 'true,' to man at any stage of his knowing? As a matter of scientific history this presumption has perpetually misled him. How much ingenuity and effort has he not wasted for aeons in attempts of this sort? How persistently has he not tried to construe physical happenings on the analogy of his own agency by the method of 'animism'? How many generations of scientific men have not insisted with Aristotle that the effective clue to nature must lie in the qualities which things have for our senses, and tried to build a system of physics out of the antitheses of hot and cold, dry and moist, heavy and light, &c.?

In § 5 Mill gets much nearer to a right estimate of Hypothesis. He disallows Newton's dictum *hypotheses non fingo*, and declares that "nearly everything which is now theory was once hypothesis". He points out that hypotheses are necessary *to guide experimentation*, for "even in purely experimental science some inducement is necessary for trying one experiment rather than another", that "the process of tracing regularity in any complicated, and at first sight confused, set of appearances is necessarily tentative: we begin by making *any* supposition, *even a false one, to see what consequences follow from it*; [1] and, by observing how these differ from the real phenomena, we learn what corrections to make in our assumption. The simplest supposition which accords with the

[1] Italics mine.

more obvious facts is the best to begin with; because its consequences are the most easily traced. This rude hypothesis is, then, rudely corrected, and the operation is repeated . . . until the deductive results are at last made to tally with the phenomena."

In the first of these remarkable passages Mill recognizes that it is not enough to conceive truth as awaiting discovery and the mind as arriving at it by the systematic exploration of the totality of reality. Reality is too vast to be exhausted thus in a finite time by minds which cannot afford to fritter away their brief span of life in aimless 'fool' experiments: hence the enormous importance of the sagacious hypothesis which takes them straight into the vitals of a problem. It is recognized, secondly, that the real guarantee of a good hypothesis does not consist in its conformity with abstract rules, formulated in advance of the investigation; the hypothesis is made good by being progressively knocked into shape in the process of its verification by the facts it has to account for. Hence it is not really requisite that a hypothesis should be correct, complete, or even probable, to start with; if it is plastic and corrigible, it will gradually assume a more and more valuable shape. The one essential it must have is that it must suggest a *convenient method* of exploring the subject it concerns; provided it does this, it may permit itself the use of the most patent fictions, and may leave aside, as 'difficulties' to be dealt with later, the incoherences which its incompleteness involves. Thus once the physicists had been led by the analogy between the propagation of light and that of waves in a liquid to assume a 'luminiferous ether', they did not allow the contradictions which arose out of conceiving this ether, either as continuous or as atomic, to deprive them of a valuable working hypothesis. It is simply not true that gaps and contradictions are fatal to the scientific status of a hypothesis, so long as it remains the best available and a hope remains of finding a remedy for its defects.

In § 6 Mill rightly points out that in the actual state of our knowledge more than one hypothesis may be competent to explain the facts, and that the correct anticipation and prediction of results subsequently found to occur in fact, though impressive, is not strictly a 'proof' of a hypothesis.

Both points are important, and have been too little regarded. The coexistence of alternative interpretations, about equally

capable of explaining the facts, and consequently in dispute, is common, and is indeed the normal condition in the growing stratum of every science: usually indeed it does not last, as regards any particular scientific question, because new facts accrue which do not remain neutral, but reinforce one side until it wins a definite superiority. If Mill, however, had taken his illustrations from the hypotheses of philosophy, he would speedily have perceived that a conflict of hypotheses need not end in any decisive victory of either side. For philosophy is full of problems which persist from generation to generation, and are as far as ever from a solution by common consent, because no facts can be discovered which cannot be made to fit into either of the rival hypotheses. To cite only two clear cases, no facts have ever been found, and probably none can even be conceived, which would decide the dispute between determinism and indeterminism or between pessimism and optimism.

Similarly, Mill is essentially right when he says 'it is not a valid reason for accepting any given hypothesis that we are unable to imagine any other which will account for the facts'. For verification neither is, nor can it ever become, proof absolute. It is committed by its method to the formal flaw of Affirming the Consequent: [1] when it argues that a hypothesis is true because the facts that follow from it are observed, this lacks cogency, because these same facts, together with others still unknown, might follow still better from another hypothesis not yet formulated. And we cannot lay it down that a hypothesis shall only be accounted true when it alone can account for the facts. For even though it had vanquished all its rivals, which seems to be an impossibility in many fields of inquiry, it would still only hold its title as the *Rex Nemorensis* held his priesthood. It would still be liable to the challenge of new hypotheses to come, and sooner or later, it would become obsolete and go under. All that can be said, therefore, for a hypothesis which successfully holds the field is that, though it is not absolutely proved and cannot claim absolute truth, it can be accepted as true provisionally and *donec corrigatur*. And is not this, after all, the most sensible thing to do? It is to hold it *as good as true*, and to give *it* all the practical privileges of truth, and *us* all the assurance we practically need,

[1] Cf. my *Formal Logic*, ch. xxii, § 6.

without encouraging a groundless scepticism which appeals to an abstract possibility there is nothing to support. Only we reserve to ourselves the right of exchanging our hypothesis for a better, should one become available, and are, perhaps, a shade more likely to look out for it. But what harm is there in that ?

§ 6. Dr. Whewell's extensive knowledge of the sciences and their history could not but exercise a beneficial influence on his views about the logic of science. Accordingly, (1) he is distinctly right, as against Mill, in emphasizing the 'colligation of facts' by a *new* conception as the really creative act in scientific induction ; and if Mill, instead of 'reflecting' on and elaborating established sciences, had attempted to organize a nascent science just emerging out of chaos, or to investigate a pseudo-science, he would soon have cured himself of the comfortable belief that the formation of a conception capable of apprehending the facts was any man's job, and quite a secondary achievement compared with the collection of the facts themselves.[1] Similarly, if our Hegelizing logicians had considered such problems of real knowing, they would not be so satisfied to regard the exposition of the powers and virtues of 'the Universal' as the whole duty of logic, but would perceive that the mere existence of universals was not of the slightest value to the sciences so long as nothing was said about the processes of contriving and selecting one that was relevant to, and operative on, the particular problem engaging scientific attention, and so long as the difference between the wrong and the right universal was held to be no concern of logic's. They might then perceive that, no matter how exalted was the metaphysical dignity ascribed to Universals, the universals in actual use are human conceptions, and that every conception *in its application* to the *data* is a *hypothesis*, which has to be *selected* from a number of competitors ; also that the use of universals to analyse experience is itself a vast hypothesis, of which it is wiser neither to underestimate the risk nor to exaggerate the success.[2]

[1] For illustrations of this see below, § 14.

[2] The situation is only obscured, and not bettered, by talking about 'the *concrete* universal', which is to be found in nature, and contrasted with the 'abstract' universal, which is characteristic of human thought. For though it is true that we treat real things *as if* they were conglomerations of universals, and as represented by conceptions we have devised, and though, *when we have hit upon the right ones* which work satisfactorily, we are enabled to predict and control the course of events (for many purposes), we are not on this account

(2) Whewell concerns himself with the *discovery* of the right conception which colligates the facts. It demands 'sagacity' and "commonly succeeds by guessing; and this success seems to consist in framing several tentative hypotheses and selecting the right one ".[1]

Here the mention of sagacity rules out the suggestion of any formal or mechanical or inevitable way of arriving at truth. Mental activity in the form of guessing is recognized, and the freedom to guess, and take the consequences, seems to be conceded. This is to rely on the consequences of hypotheses in the process of verification to sift them, and to establish their value *a posteriori*. The only point to which exception need be taken

entitled to assert that things really are such as we *feign* them to be. Now 'the concrete universal' is precisely a phrase for covering up this logical gap. It is intended to beg the question of the real validity of our thought, and to conceal the risk we take in our conceptual analysis of experience. It takes for granted what is really the hypothesis to be established, viz. that the relation of individual beings to the 'natural' kinds, in which we find it convenient to group them, is essentially the same as that of our conceptions and their exemplifications. That is, it tries to validate the claim of our thought to yield an accurate representation of the real by affixing the same label, 'universal' both to the real and to our method of manipulating it. Yet it is clear that there are great differences between the two cases. Thus the bond of unity which holds together a 'natural kind', if and in so far as we can really trace it *in rerum natura* and cannot be driven to confess that it is merely a convenient grouping for human purposes, is certainly very different from the connexion between the examples of the same conception. In a natural kind, like the 'species' of biology or the 'elements' of chemistry, the generic identity of its members appears to be constituted by a common descent and to be arrived at through a process of generation (or some analogue), in which an initial unity is progressively differentiated and dissipated by individual variations. In an 'abstract universal', the unity is arrived at by a *feat* of the mind which sets aside the recognized differences between cases as *irrelevant* (or non-significant, or unessential). In view of this and other differences it is not easy to justify the application of the same name 'universal' to both cases, especially by a logic which professes the ideal of 'the whole truth and nothing but the truth', and so debars itself from discarding *any* differences as '*irrelevant*'.

The original form of the hypothesis of real universals, as it appeared in Plato, was more modest or more honest. For Plato, while fully convinced of the validity of universals and of their superior reality, did not ignore the recalcitrance of phenomena. He refused indeed to regard it as something *more* than the universal, which alone had true being; it was condemned as a detraction from Being, as a taint of Not-Being, which corrupted the Being of the sensible world into Becoming, and delivered it over to change and decay. But he never attempted to slur over the discrepancy between the ideal and the sensible by a mere label.

[1] *Novum Organon Renovatum*, p. 59.

is the implication that there is *one* hypothesis which can be treated as *the* right one and that this is available for selection. Instead of ' the right one ' we should read ' the *best* one '.

(3) Creativeness in inventing hypotheses, and sagacity in selecting among them, are so essentially the secret of scientific discovery that this error is hardly serious, especially as Whewell is careful to note that "to try wrong guesses is with most persons the only way to hit upon the right ones".[1] He is emphatic (p. 80) that "the framing of hypotheses is for the inquirer after truth, not the end, but the beginning of his work", and that he must be "diligent and careful in comparing his hypotheses with the facts" (ibid.). "The discoverer has thus constantly to work his way onwards by means of hypotheses false and true" (p. 83), continually to test them. As such a test Whewell mentions the power to predict results, which shows that "the hypothesis was valuable, and, at least to a great extent, true" (p. 86). Where such predictions extend to cases of "a kind different from those which were contemplated in the formation of our hypothesis" and a "consilience of inductions" occurs, the conviction of the truth of a hypothesis becomes irresistible.

(4) Whewell duly emphasizes the necessity of taking risks. "Advances in knowledge are not commonly made without the previous exercise of some boldness and licence in guessing" (p. 79). He condemns the "barren caution which hopes for truth without daring to venture upon the quest of it" (p. 126), and does not even "gain its end, the escape from hypothesis" (p. 127).

§ 7. From Dr. J. Venn may be culled not a few valuable hints about the nature of Hypothesis.[2] (1) While distinguishing hypotheses from guesses by the seriousness and importance of their subject, he identifies it, or ' Supposition ', as an *attitude*, that of putting forward a conception or mental picture tentatively and doubtfully, in the hope that it may turn out to be true.

(2) He traces back "this important and far-reaching procedure" to "the necessities of daily and primitive life", to "that very familiar state of things in our practical life in which we are in doubt between two or more alternatives". It is this situation which generates "the use of the particle ' *if* ' : in other words, the use of the hypothetical proposition". But "starting from this

[1] *Ibid.*, p. 79.

[2] *The Principles of Empirical Logic*, chap. xvi.

narrow practical application, the same form has developed and adapted itself to far wider uses of science and of fancy ".

(3) So it can be used to unravel a historical problem as well as to forecast an action, and so in its 'constructive' (or rather *reconstructive*) use becomes " a method of inquiry or investigation ".

(4) " Once started on the career of framing hypotheses . . . we find it difficult to stop." Hypothesis goes on to embrace both Analysis and Synthesis, because it employs itself in separating wholes into parts, and reconstituting them in new wholes.

(5) Even if Hypothesis be taken more narrowly, no answer can be given to the question—What hypotheses are allowable in science ?—without reference to the *purpose* of the hypothesis. " The cautions against rashness in framing hypotheses " are on a par with such " sanitary advice as the injunction not to over-tire oneself ", and though " the presumption must of course always be against the man who advances a really novel hypothesis ", it " should not take the form of denouncing him for making it ". " All that is wanted is not restriction of the hypotheses that are made, but only reticence in such as are published or declared."

All this is excellent, as is Professor Carveth Read's comprehensive regulation that no hypothesis " is of any use that does not admit of verification (proof or disproof) by comparing the results deduced from it with facts or laws. If so framed as to elude every attempt to test it by facts, it can never be proved by them, nor add anything to our understanding of them ", and his inferences that " to be verifiable an hypothesis must be definite ", but that " except this condition of verifiability, and definiteness for the sake of verifiability, without which a proposition does not deserve the name of an hypothesis, it seems inadvisable to lay down rules for a 'legitimate' hypothesis ".[1] It is, of course, sound sense to demand that a hypothesis should have a definite meaning, but strange that it should be necessary to insist on this. However, no one who has noted how rarely logical doctrines are applicable to actual knowing will dispute the wisdom of this stipulation.

§ 8. Our critical survey of what is said about Hypothesis by the different sorts of logicians should not only have given us some idea of the problems involved, but also have paved the way

[1] *Logic*, pp. 269, 270.

for a more constructive treatment. This will naturally start from a consideration of Hypothesis in its most comprehensive sense, as a mental attitude or distinctive activity. For it is here that the primary difference lies between enunciating a fact and entertaining a hypothesis. In the former case alone is a (formally absolute) claim made to propound a truth that is a reflection of reality; in the latter, no such direct relation to reality is alleged. This claim may, of course, be false, and indeed often is, but when it is made, not *pro forma*, but with full conviction, it seems to carry with it a feeling of assurance, highly valued by dogmatic minds, and a pretension to finality, which is calculated to close all discussion of the question. Or, more commonly, it means that nothing questionable has yet been discovered, and that critical reflection on the apparently given 'fact' has not yet begun. As a rule it is the attitude of the positive, prosaic mind, for which all is 'fact' or nothing at all, which does not discriminate between 'facts' and appearances and can literally *see* nothing but facts, and is incapable of illumining and extending them by the play of fancy and imagination, because it shies at questions and detests doubts. This is a very useful type of mind for many mundane purposes, though it is not really adequate to cope with a world full of deceptive appearances, in which everything genuine can be imitated, and everything is capable of *camouflage* and mimicry, for defence or aggression: for the advancement of knowledge, however, its use is very limited, and as a type of mind it is normally to be put on a *lower* intellectual level than that which is able to function hypothetically. It is also to be regarded as an *earlier* type, though, as Dr. Venn pointed out (§ 7), practical life must very soon have given men occasion to develop their imagination, to assume the attitudes of inquiry and doubt, to consider alternatives and to choose among them.

The mental attitude which entertains hypotheses on the other hand, and can take 'fact' as hypothetical and possibly unreal, means an intellectual revolt against mere givenness. It has become critical of appearances, and has partially freed itself from the oppression of brute fact. It meets reality with an active response, and does not merely submit to whatever comes along. It feels free to anticipate reality by its guesses, to question it, to experiment, to distrust and doubt appearances, to rearrange the world, at least in thought, to *play* with it, and with itself. For

Hypothesis is a sort of game with reality, akin to fancy, make-believe, fiction, and poetry. In the hypothetical attitude 'facts' have ceased to be accepted at face value, to be just fact, and become capable of being *symbols*, whose suggestions are more important than their bare existence. Whatever they may be in reality, they are no longer *fixed* in the mind, but afloat; not being fixed ideas, they can be moved about and played with. But, like games in general, this play has a serious function. By loosening the connexion between what the real is (or seems) and what we think about it, it enables us to think it other, and *better*, than it is; and so, guided by our hypotheses and 'ideals' (which are postulates), we can set to work to *make* it other, and better, than it was.

Moreover, it is by this hypothesis-building habit that science touches poetry on the one side, and action on the other; for it is akin to both. (1) The play of fancy and the constructive use of the imagination reveal the creativeness of human intelligence; by their use the scientist becomes a 'maker' like the poet, and surprises the secrets of nature long before he is in a position to prove his hypotheses. In both cases human power is limited, often wofully enough; but it suffices to infuse into the *routine* of nature a breath of *novelty* that keeps it fresh and capable of progress. (2) Yet on the other side, this hypothetical attitude mediates between thought and action, and helps to break down the superficial distinction between the theoretic and the practical. It drives the scientist out of the purely receptive attitude, and makes him a *doer*. For to entertain a hypothesis is to hold a mental content hypothetically, and this is to hold it experimentally, which, again, is to operate on it and to manipulate it. And this is surely to *act* on it, though the alterations we enact by our thought-experiments cannot be rendered as visible as the changes wrought in the contents of a test-tube by a chemical experiment. But their magnitude may be estimated by comparing the crude *data* from which a science starts with the scientific facts into which they are transformed. They differ usually far more than do the rainbow and the drops of water to which it is traced.

§ 9. It will be evident from the above sketch that logicians have good reason to fight shy of the hypothetical attitude. It is not a thing to be easily caught and tamed by rules and regulations. It is not a procedure that lends itself to formal description. Its

reasoning cannot possibly be made to seem ' valid '. Nor can it be made ' safe '. It inevitably takes risks, and so must be condemned as ' dangerous '. Of course, it *is* dangerous, with all the danger inherent in life and action. But does this justify its condemnation ? Only, surely, if there exists a safer and a better way for logic to pursue. But logic can hardly any longer cling to two of its favourite delusions which prompted it to condemn Hypothesis. The first is that formally valid reasonings are *possible* ; the second that they are *safe*.

Now to the first it must be objected that *no* reasoning can be ' valid ', because none can be put in a form so valid that it will actually be valid in *all* its applications. No perfection of syllogistic form, no super-excellence of any universal ' essence ', will yield an absolute guarantee that the conclusions deducible from them will not on occasion *fail in fact*. The universal rationality of man is quite inadequate to guarantee actual rationality in the case of every lunatic, or even of any one whatsoever under stress of circumstance. The universality of quadrupedality as an attribute of the dog will not prevent a particular dog from having to hop about on *three* feet ; nor, to take graver instances, will the complete success of the law of gravitation in accounting for astronomic movements guarantee its application to the intramolecular motions of atoms and electrons. In short, every ' law ', formula, or ' universal ', runs a risk of failure when it is applied to a new case ; and is not every *further* case to some extent a *new* one ? In all ' deductive ' proof something essential is *added* to the conclusion it anticipates, by the observation that it also actually occurs. It is only empirically that we learn whether or not the laws and ' universals ' we are employing apply to the ' case ' we have selected.[1]

As, in this way, we can *never* be sure in advance that a formally valid reasoning will not in actual use develop a defect we may classify, alternatively, as an ' Ambiguous Middle ' or a ' Fallacy of Accident ',[2] it follows that *no* formally valid argument has a right to regard itself as (absolutely) safe. And it may further be pointed out that we cannot avoid running risks by sitting still and refusing to budge. For if we will not take any ' unsafe ' step, we shall assuredly miss the new truth that may be won by specula-

[1] *Formal Logic*, chap. xvi, § 10.

[2] Cp. *Formal Logic*, p. 200.

tion, and undoubtedly run the risk of remaining stuck in whatever error attaches to our actual ' knowledge '.

It is no use quarrelling, therefore, with the hypothetical attitude, and the risks it involves. It is an attitude life, logic, and science all require of us, and there is no way of avoiding risks in any of them. If our consciousness of this becomes oppressive, we can bethink ourselves of the consoling thought that life also requires us at times to *forget* our hypotheses, or to take them as *practical certainties,* when we have to act on them.

§ 10. When Hypothesis is taken in this very comprehensive sense it becomes clear why it is not to be confined to a single chapter of inductive logic, nor to be satisfied even by making much of the hypothetical judgement and interpreting universal propositions as ' hypothetical '. Allied with the volitional attitude of postulation, Hypothesis reveals the essential creativeness of human intelligence. It pervades all mental life and penetrates every logical notion. Accordingly its workings can be traced throughout every topic of logic. " If every ' fact ' rests on selection and involves an experimental analysis of the given, and if every ' law ' is provisional and in need of confirmation, it follows that there is something hypothetical about every act of thought, and that the distinctions between fact, interpretation, theory, hypothesis, and guess are plastic and fluid." [1]

Similarly, it may be shown (1) that the syllogistic (or any other) form of inference is nonsense unless the premisses are taken as hypotheses, and (2) that in their scientific use conceptions *are* hypotheses, and that in the absence of good hypotheses, *alias* ' suitable conceptions ', a science makes no progress.

§ 11. Ever since Aristotle, logicians have been assuring the students of their science that scientific reasoning, in order to demonstrate its conclusions, must start from *true* premisses. Yet from the first this dogma has involved them in the most serious embarrassments. It became necessary to explain how the sciences came by their requisite supply of initially certain premisses ; but the doctrine of self-evident and intuitively certain principles devised to supply this need, was philosophically untenable,[2] and flagrantly false to the facts of scientific history. The sciences do

[1] *Formal Logic*, chap. xxii, 3.

[2] *Formal Logic*, chap. xviii, § 3 ; *Scientific Discovery and Logical Proof* in these *Studies*, vol. i, p. 243 f.

not *start* with an assurance that their principles are true, but gradually acquire it as they progress, select their principles, develop their conceptions, and ' scrap ' their failures.

Moreover, even after this difficulty was supposed to be surmounted, it was still necessary to explain how a conclusion deduced from premisses known to be true could have any cognitive value. The form of ' demonstration ' soon incurred the charge that its conclusion was either already included in the truth of the premisses, or else begged the very point it claimed to ' prove '. The syllogism was either a tautology or a *petitio principii*. If in laying down the principle that ' all men are mortal ' we *have* included the case of ' Socrates ', the conclusion that ' he is mortal ' is already known and is nothing new, so that the ' demonstration ' is superfluous : if we have *not*, we are not entitled to assume that he is, and the demonstration begs the question. After much beating about the bush, the ' orthodox ' logics have of late agreed to say that the way out of this deadly dilemma is so to interpret the major premiss as to disclaim for it any assertion about individuals, and to take it as the enunciation of a law of nature or a ' universal ', which by the minor premiss is applied to the case in hand. But, as I have shown,[1] this interpretation is no way out of the difficulty. It merely requires the dilemma to be slightly re-worded. The interpretation in question does indeed avoid a *verbal* reference to particular cases in the major premiss. But it does not escape thereby. For it covertly assumes both that a principle or universal *can* apply to cases without going wrong, and that it *does* so apply to the case in point. When this assumption, however, is questioned, it collapses, precisely as did the other interpretations which are now abandoned. Thus, however strenuously we insist that *all men are mortal* really means *humanity entails mortality*, and *Socrates is a man* really means *humanity is predicable of Socrates*, we cannot declare him mortal on the strength of these premisses, unless it is true and certain that they apply to him for the purpose (whatever it is) of the particular argument. We cannot rely on the verbal identity of the middle term, but have to show that humanity is predicated of Socrates in the minor premiss in precisely the same sense as it entails mortality in the major. And about this a question can always be raised.

[1] *Formal Logic*, chap. xvi, § 9.

Hence, as was pointed out in § 9, we can never know with absolute certainty whether our syllogism is really sound and our conclusion will in fact follow. But even if we could know this, and know it in advance, we should only have proved that we *did* know this. If we knew then that human mortality covered the case of Socrates, the ' inference ' about his mortality proved nothing new : whereas, if we knew it not, but nevertheless asserted a conclusion which implied it, we should simply have begged the question.

There is, however, an easy and reasonable way out of the whole puzzle. Instead of taking the premisses as *certain*, we may take them as *hypothetical*, and construe the whole ' demonstration ' as an experiment with, and test of, their truth. The conclusion will then be conceived as an anticipation of experience, and its empirical verification will be a confirmation of the truth of its premisses. It then becomes easy to conceive the whole argument as what it always must be in an actual context, viz. as an incident in an inquiry relevant to some doubt or dispute. And then there will be no ground for any charge of *petitio* or lack of novelty. For we shall not have *assumed* that the conclusion is certain to come true ; we shall merely have prepared ourselves for the *news* of its coming true, if it should do so, and have enabled ourselves to *explain* it, by deducing it from premisses of which it helps to augment the credit.

This is by far the most natural and intelligible interpretation of ' demonstration ', and the only one that does not discredit its very form. There should be all the less difficulty about accepting it, seeing that the alleged need for true premisses is an entire delusion. There is no difficulty whatsoever in reasoning from any sort of premiss, false, hypothetically true, or more or less probable. The reasoning proceeds just as well whether the premisses *are* true or merely *supposed* to be. And it is quite possible to start an inquiry with doubtful, and to conclude it with assured, premisses. We have merely to admit that the empirical truth of conclusions reacts on the premisses from which they were drawn, and strengthens their claim to truth.

That hypothetical reasoning is quite feasible was admitted by the first philosopher to recognize hypotheses *eo nomine*. Plato, in the *Republic*, Book VI, has a brilliant sketch of the relations of science and metaphysics, in which he declares that scientific principles are essentially ' hypotheses ' in need of proof. This

proof they can only get if they can be derived from an ' unhypothetical principle ' which ' takes away the hypotheses ', by attaching them to the self-evident supreme truth of the ' Idea of the Good '. Until they have been thus derived, they are neither certain nor intelligible, and so it is the function of the philosopher thus to derive them. He proceeds, however, from these same ' hypotheses ' in his ascent to the First Principle. He treats them literally as ' hypotheses ' ( =things *laid down*, supports to stand on), i.e. as ' stepping-stones and starting-points ' he traverses on his way to the point at which the all-embracing, all-explaining, self-proving, and self-evident Good reveals itself to his intelligence, and he, when he has grasped it, is guided back by it to the lowest limits of the intelligible world, deducing and assuring every step, and so the original ' hypotheses ', by its means.

Now, up to a point, this is an excellent account of the procedure by hypotheses. They are essentially ' starting-points and stepping-stones '. It is true also that scientific principles are initially hypotheses, but do not remain so. Where alone Plato errs is in assuming that they can be converted into full truths only by a metaphysical deduction. The whole cogency of his argument depends on this assumption that there is no other way of establishing a principle. But this is false in fact. Principles can also be established *empirically*, by the success of their working. Hence the sciences do *not* depend upon metaphysics, but upon their own achievements in modifying, improving, and correcting their initial assumptions, until their hypotheses grow into theories, consolidate into ' facts ', and become adequate to the problems of each science. A ' hypothesis ', then, can *change* its logical character in the natural progress of a science.

§ 12. We have already suggested (in § 4, note) that the analysis of the flow of events by means of conceptions may be regarded as a great cognitive hypothesis, and have seen (in § 7) that to suggest a ' suitable conception ' for the apprehension of a subject, which is a really creative act of thought, must be at first hypothetical. Both points, however, need some elaboration and illustration.

The first is a statement of the assumption that reality is knowable, and can be known by us. This assumption is of course indispensable, and is made in every inquiry. But it is a mistake to represent it on this account as an ' *a priori* necessary truth '.

For it is only made *methodologically*, and as a working hypothesis. Not, that is, as a method which is *certain*, but as one which is *worth trying*. If the course of events persistently conspired to defeat it, and we were in consequence unable to use it to *advantage*, we should be forced either to devise another assumption or to despair of 'knowing'. Actually it is successful enough to be deemed a legitimate *postulate* and a *methodological assumption* so convenient that we are resolute to use it. But in spite of its merits and attractions, we can still perceive that it has not wholly lost the hypothetical character it started with.

§ 13. The second point demands fuller treatment. To show that conceptions are, in scientific use, hypotheses, two courses are open to us. We may either argue from the general character of their use or may illustrate concretely from the specific difficulties of various sciences. The former method will prove convincing, if we stop to consider the problem of how among the mass of conceptions, good, bad, and indifferent, appropriate and futile, which all appear to be, in a general way, relevant to a particular problem in a science, a selection is made of those which are actually tried. Clearly, this problem cannot be passed over—as it has been. The logician ought to warn the scientist that he must make a selection, and that if he selects wrongly, he will fail. Also, that it is his duty to select the *best* conceptions, if he wishes to advance his subject. Hence it is clear that since any conception used is selected, and may be selected badly, or not so well as it might be, the scientist is always running a risk of error and failure, and acting on the *hypothesis* that he *has* picked the right, or rather the *best*, conceptions for his purpose. As this hypothesis is never fully proved—and indeed is always in the long run disproved by the progress of the science—it follows that the conceptions of all the sciences should be entertained hypothetically.

§ 14. The illustrations of the second method are potentially infinite. For every science has been held up, at some stage or other of its career, by the lack of conceptions that would give it a real grip of its subject-matter and enable it to analyse it and to deduce consequences which could be verified in fact. In some sciences this condition has been chronic, but in others it has only proved a temporary obstacle.

At the moment such an obstacle appears to exist in what has been, for the last three hundred years, a very progressive science,

physics. The following account of its trouble is taken from a lecture on 'Radiation and the Electron' by Professor R. A. Millikan of Chicago University, which appeared in *Nature*, Nos. 2534 and 2535 (May 1918). He begins by saying that "recent developments in the domain of radiation are of extraordinary interest and suggestiveness, but they lead into regions in which the physicist sees as yet but dimly—indeed more dimly than he thought he saw twenty years ago"—for the reason that "experiment has outrun theory, or better, guided by erroneous theory, it has discovered relationships which seem to be of the greatest interest and importance, but the reasons for them are as yet not at all understood". Hence, "one of the great unsolved problems of modern physics". It concerns the theory of light, and arises out of a phenomenon, by no means rare in science, and common in philosophy, viz. that a hypothesis, which at one time seemed to have been defeated and destroyed, is revived by subsequent developments, and resumes the contest.

In this case the corpuscular theory had been progressively worsted by the ether-wave theory, because, unlike the latter, it could explain neither the facts of interference, nor the wireless waves, nor the greater speed of light in air than in water, and its speed's independence of the source of the light: so by the end of the nineteenth century it appeared to be dead, while its competitor had not only explained all the known facts, but repeatedly predicted new ones.

Then its troubles began. It was found that the X-rays detached negative electrons (or 'corpuscles') from about one in a thousand billion of atoms in the space they traversed—why not from the rest? Similarly, ultra-violet light falling on a metal was found to expel negative electrons. Further, the energy with which these electrons were projected was found to be the same whatever the intensity of the light that liberated it, and the same was found to hold also in the case of X- and $\gamma$-rays. These new facts did not fit in with any wave-theory, but were suggestive of a corpuscular theory. "For if the energy of an escaping electron comes from the absorption of a light corpuscle, then the energy of emission of the ejected electron ought to be independent of the distance of the source, as it is found to be, and furthermore, corpuscular rays would hit only a very minute fraction of the atoms contained in the space traversed by them. This would explain, then, both

the independence of the energy of emission upon intensity and the smallness of the number of atoms ionised."

Sir J. J. Thomson first tried to mediate between the two conceptions by "assuming a fibrous structure in the ether and picturing all electro-magnetic energy as travelling along Faraday tubes of force conceived as actual strings extending through all space". This hypothesis, mythical as it sounds, and unable as it was to explain the facts of interference, was carried further by Professor Einstein in 1905. He "assumed not only that the energy emitted by any radiator kept together in bunches or *quanta* as it travelled through space, but also that a given source could emit and absorb radiant energy only in units which are all exactly equal to $h\nu$, $\nu$ being the natural frequency of the emitter, and $h$ a constant which is the same for all emitters". It followed from this assumption that "the energy of emission of corpuscles under the influence of light would be governed by the equation $\frac{1}{2}mv^2 = ve = h\nu - p$, in which $h\nu$ is the energy absorbed by the electron from the light-wave or light *quantum* . . . $p$ is the work necessary to get the electron out of the metal, and $\frac{1}{2}mv^2$ is the energy with which it leaves the surface". At the time when this formula was constructed there was no experimental evidence to support it, though it had been suggested by some experiments of Professor Planck's; but it has now resulted from ten years of laboratory work that "in the discharge of electrons by light this equation of Einstein's seems to predict accurately all the facts which have been observed". Hence "it must certainly be regarded as one of the most fundamental and far-reaching of the equations of physics . . . for it must govern the transformation of all short-wave-length electro-magnetic energy into heat energy".

In spite of this success, however, "the semi-corpuscular theory, out of which Einstein got his equation, seems to be wholly untenable, and has, in fact, been pretty generally abandoned, though Sir J. J. Thomson and a few others seem still to adhere to some form of ether-string theory". Two objections are fatal to it: it cannot account for the facts of interference, and there is positive evidence against any fibrous structure in the ether. The 'oil-drop' experiments have shown the granular structure of electricity by discontinuous changes in the velocity of an electron when the charge in it is varied while the electric field remains constant; but the converse experiment shows the *lack* of a discontinuous

change, when the charge remains constant and the field is varied, and has disproved the fibrous structure of the field. " Despite, then, the apparently complete success of the Einstein equation, the physical theory of which it was designed to be the symbolic expression is found so untenable that Einstein himself, I believe, no longer holds to it, and we are in the position of having built a very perfect structure, and then knocked out entirely the under-pinning, without causing the building to fall. It stands complete and apparently well-tested, but without any visible means of support."

Professor Millikan then proceeds to consider whether the explosive emission of energy by atoms in definite *quanta* cannot be ascribed to a gradual absorption of energy inside the atom. " It is necessary to assume, if the Thomson-Einstein theory is rejected, that within the atom there exists some mechanism which will permit a corpuscle continually to absorb and load itself up with energy of a given frequency, until a value at least as large as $h\nu$ is reached. What sort of a mechanism this is we have at present no idea." It entails " a type of absorption which is not due either to resonance or to free electrons. But these are the only types of absorption which are recognized in the structure of modern optics. *We have as yet no way of conceiving this new type of absorption*[1] in terms of a mechanical model ". Still, this type of explanation, " though as yet very incomplete, seems to me to be the only possible one. . . . Yet the theory is at present wofully incomplete and hazy. About all we can say now is that we seem to be driven by newly discovered relations in the field of radiation, either to the Thomson-Einstein semi-corpuscular theory, or else to a theory which is equally subversive of the established order of things in physics ". Manifestly, then, the path of physics is here blocked, until some *new* conception, suggested, it may be, to some creative mind by some further experiment, pronounces its ' Open, Sesame ! '

§ 15. As a good example of a science hung up for ages, in a manner strongly suggestive of a lack of appropriate conceptions, we may consider the sad case of Psychology. Here we have a science of apparently enormous potentialities and pretensions, of universal interest, of great antiquity, upon which many

[1] Italics mine.

generations of thinkers have lavished much time, ingenuity, and enthusiasm. Yet disappointingly little has been made of it. After more than two thousand years of strenuous cultivation, it still has no laws but only technical terminologies, no *consensus* about methods and principles but a swarm of discordant ' schools ', no definite limits and no assured territory but far-reaching claims and perpetual border-wars with all its scientific neighbours. It has ' descriptions ', but none adequate to the subtleties and shades of the processes they describe : nor has it any real control of the mind and the power to predict its operations. Indeed, it does not seem quite sure even that it has got a mind, any more than that the ' soul ' exists, from which it draws its name. For one of its latest fashions is seriously endeavouring, under the flag of ' Behaviourism ', to construct a psychology from which the conception of consciousness has been eliminated altogether.

The obvious explanation, to which this condition prompts, is that Psychology has not so far succeeded in getting hold of appropriate conceptions. A glance at those it has tried to use would seem to confirm this suspicion. They seem to be a very ' scratch ' lot, borrowed from all sorts of quarters, ill-defined and ill-adapted to any sort of scientific testing, and never reducible to systematic order and logical coherence, except at the cost of suppressing half the facts they are supposed to account for.

For example, it would seem that Psychology has never been able to make up its mind even on the fundamental issue whether the mind is one or many. From the first (Plato), eloquent asseverations of the indiscerptible unity and simplicity of the mind or ' soul ' or ' self ' have been jostled by elaborate classifications of its division into ' faculties '. Psychology has always been fond of faculties, with a foolish affection, and after every official disclaimer of their value may always be caught reinstating them more or less covertly, though it has never been shown how any mechanism of faculties, whatever its complexity, could explain even the simplest mental operations,[1] or even how any ' faculty '

[1] The advocates of ' faculties ' always have finally to admit that the analyses effected by their aid are illusory, because actual psychic life always actuates them all. Instead, however, of regarding this fact as a confutation of their method, they only feel prompted by it to descant on the superior unity of the mind. But if the mind is so perfect a unity that none of its faculties will ever operate singly, what was the use of feigning a classification which divided it into a plurality of radically different faculties ?

could be more than a futile *ex post facto* replica of the function it professed to explain. No doubt the source of these embarrassments is that, for *various purposes*, it *is* imperative to treat the soul, now as one, and now as many. But Psychology has never been able to make clear what is the scientific case for this variety of purposes, or how they are to be brought into accord with each other. A sympathetic critic must conclude that no conception suitable for expressing simultaneously both the unity and the plurality of psychic life has yet been devised.

Again, he will not be able to resist the conviction that the analogies from the external world, with which, as a nascent science, Psychology was forced to commence, have served it ill. The analogies of the ' spirit ' and ' psyche ' (soul) with a *breath* (*spiro*, *ψυχᾶν*), and of the mind or soul (*animus*, *anima*) with a *wind* (*ἄνεμος*) were not really suited to express the uniqueness of its nature. All the comparisons of the soul with an external ' *thing* ', which prompted primitive man (logically enough) to tell fairy-tales about the wicked giant who kept his soul for safety's sake in the guise of a bird in a cage, or locked up in a box, in the strong-room of a castle in an inaccessible island in a remote lake well out of harm's way, and scientific psychologists to fabricate, less picturesquely, pseudo-sciences about the ' combinations ', ' complexes ', ' complications ', ' associations ', &c., of fictitious ' elements ' that (unlike the giant's ' soul ') always eluded capture, have proved fallacious and unworkable. They failed because soul-structures are not permanent, like rocks and houses, or even trees.

But even if we learn, from these failures, more wisely to express our psychological hypotheses in *functional* terms, and to speak of ' processes ' and ' attitudes ' and ' activities ', instead of ' soul-substances ', ' sensations ', and ' ideas ', we are still only groping towards an adequate expression of the subject's singularity. The same objection holds more or less against all the conceptions, schemes, and technicalities of psychology—*they do not work.* But this is no reason why the psychologist should despair. He should recall rather how long and fruitlessly sciences like physics have had to struggle before they arrived at their present scientific rank, and should redouble his efforts to bethink himself of something new and suitable, opening his mind to suggestions from every *imaginable* quarter, and trying the hypotheses which occur to him, not by his antecedent prejudices, but by the success of their

applications. Only there is *one* condition he must not overlook, in his satisfaction with the comprehensiveness, symmetry, and beauty of any system of psychology he may devise: whenever he has discovered or invented a set of conceptions which seem to him adequate to his subject, he must not shrink from proving the correctness of his *analysis* of the mind by a synthesis, and must show himself capable of *reconstructing his own*. For no scientific doctrine must be allowed to evade the test of application.

§ 16. A just appreciation of the functions of Hypothesis in the widest sense, and of the value of the attitude of supposal, will naturally conduce to greater sympathy with the non-scientific forms thereof, as well as to leniency in regulating those which are employed in scientific enterprise. For we shall then see that it is quite right and proper for the products of this creative attitude to differ according to the purpose for which they are employed and the nature of the subject they concern.

As for the narrower sense of Hypothesis, its function in the service of science is to *think the new*, whether it appears as an unprecedented fact, or demands the formation or reformation of conceptions. So long as it serves its purpose, much may be forgiven it. It need not be 'safe' or 'valid'; it need not accord with old analogies and time-honoured prejudices, nor be complete, and free from 'difficulties', or even 'contradictions'.[1] It need not have confuted all its rivals, in order to be accepted as the *best*; nor need it last for ever, and defy change.

The sole essential of a scientific hypothesis is that it should *work*—relevantly of course to the problems of the science. This postulate, however, naturally articulates itself into three.

(1) A scientific hypothesis must have a *definite meaning*, as was said in § 7. This does not mean that its terms should be conceived as rigid and unchangeable, but that its meaning must at any rate permit of deductions being drawn from it, and put to the test of experiment. In other words, a hypothesis which cannot be applied means nothing—scientifically. Such hypotheses are rare in the stricter sciences, but common enough in ethics and politics, and rampant in philosophy, where theories have a strong tendency to assume an inapplicable form, as a protection against criticism. What, for instance, is the meaning of a purely formal

[1] For apparent 'contradiction' as an incident in the growth of meanings, cf. these *Studies*, vol. i, p. 241.

law of duty, like Kant's 'Moral Law', which cries out against the degradation of every sort of application? What is the meaning of nearly all the catchwords of politics, of 'democracy', 'autocracy', 'liberty', 'equality', 'social justice'? What is the function of realities and 'essences' that are unknowable, of 'substances' that cannot appear, of qualities that are 'occult', of designs that are 'inscrutable', of truths that are 'unverifiable'?

(2) Inapplicable hypotheses, then, are meaningless just because they evade testing, and thus defy the *second* requirement of a scientific hypothesis, that *it must be such as to admit of definite tests*, which determine by their issue whether it is true or false. This proviso condemns, not only hypotheses which decline to be tested altogether, but also those which are *too* accommodating. A hypothesis which professes to explain everything, but is compatible with anything, is not scientific: for it, too, refuses to take the risk of refutation. Such hypotheses were once prevalent—in mediaeval science any event might be ascribed to 'witchcraft' or the Devil's agency—and they still linger on in philosophy, where 'the Absolute' is a palmary example of such illusory explanation. The *testing* of hypotheses is their *verification*, and is, in principle, an *unending* process. But it is needless to regulate it further by logic, because both the nature of the tests, and their value, are relative to the problems to be solved, and the actual investigators of a science alone are the competent authorities to decide these matters.

(3) The only condition the logician is entitled to suggest is that the hypothesis accepted should be the one that *works best*, i.e. better than any alternative within the purview of the science. He can urge the true scientist to be ever on the look out for the best hypothesis, and not to be obstinate in clinging to old hypotheses, to which he has become attached, when the new are better. He should, therefore, cultivate the qualities of mind which will enable him to act thus, and, in the interests of truth, try to become as tolerant and open-minded as his idiosyncrasy permits. Nor should he try to suppress the alternative hypotheses by mere authority and to repress discussion.

§ 17. For inquiry demands an abundance of hypotheses. In the beginnings of a subject, especially, it is most important to keep a long and varied list of alternatives before the mind. For only so shall we have our attention called to the variety and

intricacy of the facts, whose significance does not usually lie on the surface, and can to some extent guard ourselves against overlooking any relevant feature of our problem.

In the 'facts' alone we cannot put our trust. For (1) the facts are precisely what we have, and hope, to ascertain. The 'facts' we start with are what we have to examine, and need not be more than apparent; the 'real facts' have to be extracted from them by critical experiments and the hypotheses which guide them.

(2) It should be recognized also that the 'facts' are always *more or less relative to the hypotheses* which apprehend them, and in terms of which they are described. They take their colour from the hypotheses they serve. Violently antagonistic hypotheses will appeal to unrecognizably different 'facts', and to get an adequate supply of facts we may need a plurality of hypotheses.

(3) Yet the facts themselves, in cases where we can get them apart from hypotheses, are often amiable enough. They will display a charming *ambiguity*, and fit into several hypotheses with (approximately) the same facility. We have to labour therefore to overcome their flabbiness, and to find facts sturdy enough to withstand assimilation by hypotheses, and to lend themselves to 'crucial' experiments. It is precisely because we cannot find such facts that so many of the ultimate issues of philosophy cannot be decided.

For all these reasons, then, science should habitually reckon with a plurality of hypotheses, and eschew the sharp antithesis between 'the right' hypothesis and the many 'wrong' ones. Its concern is really with the *relative values* of the hypotheses in active service.

§ 18. We may conceive ourselves to have acquired in the foregoing discussions a clear conception of the function of the hypothetical attitude in scientific research, and to have liberated the latter from the severe restriction of the former in the name of logic. But it remains to reconcile the scientific attitude towards Hypothesis with the practical, and to show that they are not so discrepant as they are usually supposed to be.

At first sight it would seem that, however valuable the attitudes of doubt and inquiry, the methods of systematic questioning, the exploration and weighing of alternatives, the unending remodelling and reconstruction of beliefs, might be for the purpose of advancing

knowledge, they were at any rate entirely unsuited to the life of action. For action demands finality and decision, a choice between alternatives, a firm stand on stable principles, a suppression of doubts, a fixity of purpose, a going on resolutely on a chosen path without looking back or looking round. It must fix its eye upon its aim undeviatingly, and resist the distraction of hypotheses.

All this is true, but it is not the whole truth. For if it were, the action would be *blind*. The action, however, which our life requires is *circumspect*. It always presupposes more or less consideration of alternatives, before a judgement is passed and a decision is taken. And this process occurs equally in action and in science. There is no radical antagonism between theory and practice.

That it is not *easy* to reach equal proficiency in both stages of successful action, or of successful inquiry, may cheerfully be admitted. The good theorizer is not necessarily the most ingenious experimenter or the most careful observer. So the narrow-minded fanatic often has more driving-power than the mind that looks before and after. But it not infrequently drives him too fast, on the road to destruction. As we cannot often get a genius ' four-square without a flaw ', we have everywhere to allow for the defects of men's qualities. And the difficulty of combining these two conflicting attitudes is precisely one which life involves for us all.

It may also be admitted that the attitude of doubt, the entertaining of hypotheses, the holding of judgement in suspense, is irksome, and even intensely repugnant, to many minds. They demand *certainties*, and are eager for *assurances*. They think doubt torture. They shrink from risks, but, if they needs must take them, prefer to ' go it blind '. The traditional theories of theology, philosophy, and logic have always catered for these minds.[1] They have even pandered to their weaknesses. They have represented knowledge as proceeding from truth to truth, and not from error. They have claimed an *absolute* certainty, where all we have (or need) is certainty enough to live by. They have confounded doubt with sin and scepticism, and discouraged the attitudes of inquiry, experiment, and faith.

But these theories are profoundly inadequate, and the minds they flatter are neither the noblest, nor the best adapted to the

[1] Cf. The *Proceedings* of the Aristotelian Society, 1918, p. 267 f.

nature of the world. The opposite bias, which actually *enjoys* danger and courts adventures in the realm of thought, and has no desire to rest content with the established order, but rejoices in activity and plunges into movements, finding

> Life's treasure in an endless quest,
> And peace of mind in infinite unrest,

is no doubt too rare to extort recognition, and involves a renunciation of ultimate ideals which perhaps ought not to be lightly sacrificed. But it exists, and though *intellectual* courage is much rarer than physical, it, too, has its place in the development of the human spirit. And a spirit of *fortitude*, which, though it does not *revel* in risks, sees the necessity of running them, and is willing to accept them, should not be beyond the compass of a reasonable logic.

# SCIENCE AND METAPHYSICS

By the late J. W. Jenkinson

This paper was read by my husband some years ago to a Philosophical Society in Oxford. He had at the time no thought of publishing it; but, acting upon the advice of his friends, I have now decided to do so. I am much indebted to Dr. R. R. Marett for very kindly reading over the manuscript and preparing it for the press. This is the paper to which reference is made by Dr. Marett in his Biographical Note prefixed to my husband's posthumous work, *Three Lectures in Experimental Embryology* (Oxford, Clarendon Press, 1917).

C. Jenkinson.

Πάντες ἄνθρωποι, says Aristotle at the opening of the *Metaphysics*, τοῦ εἰδέναι ὀρέγονται φύσει. A striving after knowledge is implanted in the nature of all mankind; and all philosophy, all love of wisdom, begins in a wonder which the child feels equally with the savage—a wonder which expresses itself first in astonishment and awe at the visible mysteries of nature that surround him, and afterwards in perplexity before the invisible secrets of his own soul. As the child grows into a man this vague bewilderment is gradually replaced by a definite method of inquiry controlled by the reason; and the same process is observable in the evolution of the knowledge of the human race.

The earliest philosophers of ancient times, such as Thales, turned their attention first of all to the material universe. But, owing partly to the entire want of a scientific method, and partly to the absorbing interest of the problems of practical life, the pursuit of the physical sciences soon fell into disrepute. Meanwhile, the need felt by the more earnest thinkers of supplying some ultimate reason for the maxims and conventions that formed the moral repertory of the ordinary citizen was instrumental in evolving a stupendous system of metaphysics; and the ablest philosophies of later days have found it difficult to go beyond it.

At the same time, the study of purely mathematical sciences led to the elaboration of the syllogism. This, however, was not an instrument for the prediction of fresh instances from general laws ascertained by rigorous induction. Hence it became later in scholastic and ecclesiastical hands an engine of tyrannous oppression, which sought to enslave the human intellect to dogmas derived from patristic premisses.

But in more modern times the old order of thought has reasserted itself. Under a kindlier fortune freed from the bondage of tradition, the sciences have been enabled to develop. By the use of induction, man proceeded to inquire into the operations of nature, into the meaning of those mysteries around him that were the first to attract the attention of the primitive philosopher. Such inquiries have proved so successful, while their pursuit has been so absorbing, that those who have prosecuted them have been often tempted to suppose that solely in the knowledge obtained by these methods lies the sum total of possible human wisdom ; that in a connected view of all their results consists the true philosophy, the only attainable system of the universe.

Whether this claim be justifiable or not we shall presently have to inquire. For the moment let us confine our attention to attempting to answer the questions : What is meant by a science ? What is it in modern science that has led to such brilliant results ? and how far is there a single method common to all the sciences ?

With regard to the purely mathematical sciences of geometry and arithmetic, it is obvious that in them the deductive method alone is employed in order to develop all that is contained in the universally acknowledged, axiomatic laws of space and number. These sciences, therefore, do not, of themselves, lead to any knowledge of such events as occur in space and time, or in time only—forming the subject-matter of the sciences called inductive, with which we are chiefly here concerned.

It may very possibly be objected that it is improper to speak of an object, such as a stone, as an event. For the stone seems at first sight the permanent possessor of unalterable properties. A moment's reflection, however, will show that even such a relatively simple object as a stone is, as truly as more complex objects, but a passing assemblage of atoms. As the mineralogist and geologist would tell us, these atoms have certainly not always been so combined, are even now imperceptibly separating from

one another, and will in the future be scattered to all the ends of the earth, perhaps of the universe.

An object, then, may properly be styled an event, or better a concourse of events. Each event is determined by certain others that have preceded it, and determines in like manner those about to follow; being thus related to the past on the one side, and to the future on the other. Now it is the business of science to take those concourses of events which the ordinary man calls natural objects, and unravel the combination. It must separate as it were the constituent threads of the complicated plexus, and discover what events have determined those constituting the given phenomenon, that is to say, have always been found invariably and necessarily to precede them; and, if possible, also what events will in turn be determined by them. In the case of the stone, for instance, its position and shape, the minerals that compose it, and the chemical constitution of these have all been determined by certain definite events in the past; while all in their turn will help in determining other events such as sea-bottoms, mountains, or animals and plants in the future.

But, in order to give an intelligible explanation of an object, science must also, apart from this task of discovering what made it, and what it does, attempt an answer to the further question, What is it made of? There is, therefore, in every science an ultimate something, mass for instance, or the molecule, or the atom, or the living cell, but in all cases something, to which it is sought to reduce natural objects, and from which they are regarded as having been derived in accordance with the particular point of view of the science concerned. At the same time, every science has an ultimate conception, such as molecular motion, or chemical affinity, in terms of which it endeavours to express those particular changes in natural objects to which it pays attention.

It will thus be seen that science, in attempting to determine, in Aristotelian language, the material cause of an object, also proceeds to the discovery of the efficient cause, and vice versa; and that in both cases it is seeking for the antecedent necessary conditions of the existence of that object. When, on the other hand, we look for an answer to the question, What does the thing do? we are inquiring not into the causes but into the effects of the object—in other words, into those phenomena which invariably and necessarily follow the given phenomenon, and are, in short,

its properties or functions. When we say that an object has such and such properties, we mean simply that under certain conditions it will do certain things, will produce such and such effects. Matter, for instance, has the property of weight, that is, it will fall to the ground; copper sulphate has a blue colour, that is, affects the retina in a particular way; Canada balsam has a certain refractive index, that is, alters the direction of light passing through it at a certain angle; and so on.

All these examples have been taken from the class of inanimate objects; and, as a matter of fact, we do not commonly speak of the properties, but of the functions, of living things. In both cases alike, however, property or function means a tendency to produce under the appropriate conditions certain definite effects. At the same time we must never forget that, while we study the effects of one event, we are equally engaged in studying the causes of others. Hence, stated generally, the essential aim of a science may be said to consist in discovering the processes by which the natural objects that are immediately at its disposal have come to be derived from those simpler, and for it, ultimate elements whereto it endeavours to reduce them.

Now, unfortunately, in the method by which a science proceeds on this path of discovery, considerable confusion is, at the very outset, frequently caused by our not always referring our observations to a trustworthy standard. Sometimes we do indeed speak accurately, that is to say, quantitatively. Thus, when we say that weight is a property of matter, we mean that matter falls to the ground with a certain acceleration expressed in the easily ascertainable units of the metre and the second. In other cases, however, a property of a body is taken to mean its relation to the human nervous system, which as a measuring instrument is very delicate, easily disturbed, and highly variable. Hence the property—any colour or taste, for instance—has the great disadvantage of being merely qualitative and not quantitative, and is therefore not capable of that accurate observation and comparison which is the first essential in all sciences whatever. And, in the second place, as the arrangement in which the objects are found and observed is more or less chaotic, so also will be the table of observations, however carefully it be drawn up, and however accurately measured. This is bound to happen unless the objects are constantly compared with one another and classi-

fied—unless, that is, an attempt is made to proceed from the particular instances, so as to arrive at general conclusions. Now, when we consider what it is that has made modern science so fruitful in results, the attention is at once drawn to a principle of paramount importance. *Vere scire*, said Bacon, *est per causas scire.* The application of this maxim has abolished, to a large extent at any rate, the old vicious *inductio per enumerationem simplicem.* It led to the supplementing of mere observation by experiment, and so substituted a rational method of inquiry into the facts and operations of nature for the blind gropings and fantastic hypotheses formerly in vogue.

In order, then, to be able to arrive at such a generalization as shall be not a mere summing-up of facts already observed, but shall include hitherto unobserved instances—in short, in order to make an induction—the test of causation must be applied. Observation, the first stage in the process constituting scientific method, when it has been supplemented by experiment, and guided, in the search for new facts, by hypothesis or incompletely justified generalization, will lead to the second step, classification. The latter is equivalent to the establishment of general laws, that is to say, of statements as to what kind of events invariably and necessarily precede certain other kinds of events; whence we ascend through successively higher and higher stages to those laws of nature which in every particular science covers all the facts that come within its cognisance.

Whether there is discoverable any one law of nature such as would embrace all the ultimate laws of the particular sciences, and therefore all particular facts whatsoever, is a speculation of which the discussion may be for the present postponed.

The third process involved in scientific method is deduction—prediction, that is, that new facts, as yet unobserved, will fall under the laws already ascertained. While the laws are thus verified, the facts in their turn are said to be explained, when it is shown that they really are particular instances of those laws. In precisely the same way, in pure mathematics, theorems are said to be proved.

Let us now see how far these conceptions are being, or can be, carried out in the particular sciences.

Taking a simple example from dynamics, let us suppose we are investigating the phenomenon of falling bodies. From mere

observation of such bodies, which are seen to pass through equal spaces in very different times, we could hardly arrive at a satisfactory generalization. The Peripatetics, indeed, had formularized the law that bodies fall through equal spaces in times inversely proportional to their weights; but this proposition Galileo easily disproved by letting stones of unequal weights fall from the tower of Pisa.

If, however, experiment be introduced, say, after the manner of Galileo, or by observing the fall of bodies *in vacuo*, we find, on the one hand, that the weight, or as we should now say the mass, of the body makes no difference to the time it takes to fall, and that the observed differences in time are due to the media through which it passes. On the other hand, we arrive at the general truth that the time depends on the force of gravitation in that particular place, and on the original distance of the body from the earth. This law can be verified as often as we please by fresh observations; which are themselves explained when it is shown that they are particular instances of the law.

The causes which, in this case, experiment has succeeded in eliciting from the crude observations are all cases of the action of one body on another, either in the form of attraction, or of direct impact—forces that can be easily measured, and ascertained from the molar motions of the bodies themselves.

Again, from mere observations made on boiling liquids it might be thought that the cause of boiling was merely the direct application of heat. But as soon as it was shown, by placing liquids in an air pump and withdrawing the atmosphere, that is, decreasing the pressure, that they were thrown into a state of ebullition, it became evident that some wider explanation must be sought for the facts. Now in all cases of boiling the liquid was found to decrease in volume by the escape of vapour at its surface. The attention of observers was thus drawn to a study of the effects of heat and pressure on these vapours, and it was found that the vapour tension was constant for a given temperature, increased as this increased, and at the temperature of the boiling-point of the liquid was equal to the atmospheric pressure. Boiling therefore in all cases consisted of the equalization of the two; and could be produced either by increasing the former, or by diminishing the latter: the boiling-point of a liquid not being constant, but varying with the atmospheric pressure. Moreover, as was

seen in the previous example, if a new liquid were to be discovered, it could both be predicted and be verified that these relations hold good.

The same method is adopted in chemistry. Early observers were led, simply through the neglect of experiment, to put forward a theory that regarded combustion as an escape of the so-called phlogiston from the burning substance. The simple experiment of weighing the substance before and after its combustion of course soon upset this. Again, a very large number of substances under certain conditions emit light and heat, and these phenomena might be supposed to be identical with those of combustion. But the experiment of passing an electric current through a platinum wire *in vacuo* proved that, while both light and heat were emitted, the alteration of the substance found in all cases of combustion had not taken place. The generalization has thus been arrived at that combustion is a chemical union between two substances taking place with efficient energy to develop light and heat ; and that these are therefore effects of it, while its causes are chemical affinity and certain conditions of temperature and pressure. Here, as before, a new phenomenon would be explained if it could be shown to be a particular instance of combustion, which itself is a case of the higher laws of general chemical reaction. The necessary antecedent phenomena, however, cannot in this case, so far as we yet know, be identified with merely mechanical forces acting between bodies whose molar motions can be easily ascertained. Chemical affinity is a name for certain links in the chain of cause and effect that are imperceptible to us, but are believed in because they form our only means of explaining the phenomena. The atomic theory provides the ultimate conception of chemistry, and it is justified by the verification of predictions made from it.

What is true of the imperceptibility of the effects of heat and light in purely physical phenomena is additionally true of them in chemical ones ; for here the motions that we suppose heat, light, and electricity to induce are no longer merely inter-molecular, but intra-molecular as well.

Thus far the conception of scientific method originally laid down has been justified. In the three sciences from which examples have been taken, the knowledge attained is a knowledge of the general laws of causation—of the relation to one another of events

in time, that is, of the determination of those which follow by those which precede. When, however, we come to the consideration of biology, it is by no means so easy to say that these conceptions enter into the logic of the science, or even that, in its present condition, it has any method at all. Indeed, there are some who would have us believe that the ordinary laws of cause and effect do not operate in this sphere; and that if we wish to make certain progress we must desert this path and seek another. Now, leaving quite out of the discussion the question what life is, that is to say, what have been its determining causes, if indeed any such exist, we may, as does the chemist with the atom, take as the ultimate conception of biological science the activity of the living cell. The living cell, even in the most simple of its forms, is not a mere undifferentiated mass of protoplasm with certain chemical and physical properties. It is an organism, a body possessing a definite structure, of which the various parts, or organs, such as, say, cilia, the nucleus, or a contractile vacuole, perform in relation to the environment certain functions such as motion, excretion, or reproduction. The collective performance of the functions constitute the phenomenon known as Life, these having as their apparent object the maintenance of the individual and the species. This is as true of the higher multicellular animals and plants as it is of the lower ones. In their case, however, the organ is not a part of the structure of a single cell, but is composed of one or more multicellular tissues that have taken upon themselves special functions. But in both cases it is the physiological division of labour that makes the organism what it is; organisms in all their variety forming the subject-matter of biological science.

In definite accordance with the function it performs, every organ has a form and structure of its own. Hence, in zoology at any rate, the mass of material is so great that the science has become divided into the two branches of morphology and physiology. The latter seeks to determine by accurate observation and experiment the exact functions of the various organs. On the other hand, it is the aim of the morphologist carefully to describe the very large number of varieties of structure found in organisms and organs, not only in their adult condition, but in all stages of their development; this latter study being separately classed under the special head of embryology. Further, the morphologist attempts to classify his observations by the aid of the conception

of homology, those organs being spoken of as homologous which have similar spatial, though not necessarily temporal, relations, at least from a developmental standpoint, to the other organs and the whole organism, quite regardless of the function that they perform.

The existence of such orderly groupings can find an explanation only in the belief that the common possession of these homologous characters indicates descent from a common ancestry, a belief which finds strong support in the testimony of palaeontology. A natural classification would be one which coincided with this descent; and, if it were possible to determine the latter by other than purely anatomical means, we might perhaps know why the characters are grouped as we find them.

The scientific method of morphology, then, is to observe large numbers of facts, and to classify these in co-ordinate and subordinate groups on the supposition, unfortunately almost entirely untested at present by experiment, that this classification represents a natural descent going ultimately back to the simplest living elements. Indeed, if we ask ourselves what causes have led to all the varieties in structure and function which we see, we are bound to admit that neither morphology nor physiology has given any answer, at any rate any complete answer, to the question. Physiology, as we have seen, only determines the functions of organs, that is, the effects they produce; and, although this method, tested as it is by experiment, allows true generalizations to be made from which it would be safe to predict, still it gives no answer at all to the question which is being asked. The reason for this is that it regards the organism as the permanent seat of the same unalterable functions eternally performed for the sake of its own perpetuation, and not as something whose structure and correlated functions have had a definite history, and out of which organs of a totally different kind are probably going to develop.

In morphology the generalizations that are made are for the most part empirical. It has been found by experience that characters common to a large group of animals are not generally variable as regards their occurrence within the limits of subordinate groups, such as genera or species. We are as certain, for instance, that the next frog we open will possess an anterior abdominal vein, a character which it shares with a very great

many other animals, in other respects often widely different from it, as we are that all men are mortal. But we are quite ignorant of the causes that have led to the possession of this character. Were the question propounded, it is perhaps not improbable that a morphologist would reply, 'because the frog is an amphibian'. But this, strictly speaking, is no explanation at all, as it is hardly necessary to point out. In the bare statement as it stands, there is no more involved than that an object possesses a certain property or character, because it belongs to a class of things, of which all have been found to possess the same character. Implied, however, therein must be the belief that some common cause has led to the existence of the common character, that in fact, the frog in this case is a special instance of some general law or laws. If we knew what causes had led to the existence of an anterior abdominal vein in the ancestral amphibian; if we knew how far heredity was able to preserve an old character in opposition to the new ones constantly coming into existence; and if we knew the laws of the mutual correlation of the different organs: then not only should we know why every frog that we open has this vein, but should likewise be able safely to predict whether or no any newly discovered creature, which for other reasons must be included in the class, would possess it also.

In order, therefore, that morphology may become the science which at present it is not, it seems to be of the highest importance that organs and organisms should be regarded as events determined by definite and necessary antecedent conditions, such as are to be found in the environment, in hereditary sources, and in the mutual interactions of the various organs. If we do not accept the influence of the first as a factor in phylogeny, we shall be reduced to the '*innere Ursache*' of latter-day German biologists. If, on the other hand, we do, we have to choose between Lamarckianism and natural selection. In the latter case experimental evidence must be obtained of variation, as well as of the selection of the 'fittest'. The direct influence of the environment upon the individual has lately been investigated by experimental embryology. Moreover, all three factors of organic evolution have in recent years been submitted, with considerable success, to statistical treatment. By this means it has been possible to give an experimental demonstration of variation, of natural selection, and of the correlation between different organs in the same

individual, or between examples of the same character in successive generations.

It is necessary here to advert to a method of explaining biological phenomena which has been, and is, persistently used by a certain school of writers and thinkers, and, unfortunately, gains credit from the loose language into which biologists too often allow themselves to slip.

The doctrine of Teleology asserts that, in some way, the form which a rudimentary organ will eventually assume is determined by the function that it will ultimately perform. Now as a matter of fact there is a great deal of truth in this statement, although not if it be taken in the manner in which it is ordinarily understood. As far as can be gathered from writings of the teleologists, what is meant is that *at the time* at which a particular organ began to develop into the form in which we now find it—at the time, for instance, when the complicated auditory apparatus of the vertebrata was merely a shallow pit on the surface of the head—the preceding necessary condition of such development was the fact that long ages after it was to perform a particular function which it did not exercise originally.

As that which follows cannot in any sense be said to determine *in time* that which precedes, the theory further involves the belief that the predetermining condition was an act of volition on the part of a conscious being in whose mind there was *at the time* an idea of the function that the organ was some day to perform: an act of volition that, presumably, has been and is repeated for every separate organ, and for every stage in structure and function through which every organ has passed or will pass. Now, even if such a preceding and necessary condition existed, it would not be ascertainable by any scientific method, there being no reason to believe in the existence, say, in Jurassic times, of a sentient being capable of such volition. But those who uphold the doctrine seem to have fallen into a confusion between two different things, the formal and the final cause.

The material, efficient, and formal causes, if we mean by the last the idea of the effect in the mind of a sentient being, all precede in time the occurrence of that effect; and this kind of teleology is not, as it is asserted to be, a doctrine of final, but one of formal, causes. The final cause stands for the use to which an object is to be put, the effects it will produce, the function

it will perform, which obviously succeed in time the existence of the object itself. The final cause, then, cannot be taken as ever determining in time the existence of the object, and is therefore a conception which belongs not to science at all, but to metaphysics. At the same time it must be admitted that misleading language is very frequently employed in this connexion by biologists themselves. How often, for instance, is it said, that the form of an organ depends on its function, or that an animal has such and such a structure in order that it may perform a certain function. This statement may be perfectly true in a metaphysical sense, but it tells us nothing of the conditions which determined the origin of the animal or organ. It may be a very convenient shorthand expression for the facts. Yet, speaking scientifically, it would be more accurate, even if more circuitous, to say: the conditions in the past have been such that this particular animal or organ does now, as a matter of fact, perform this particular function, organ and function being co-ordinate effects of a common cause.

The only necessary conditions of a phenomenon ascertainable by science are those material and efficient causes which precede it. At the base of biology, then, there certainly lies the belief and hope that it may be found possible in some way to determine what these are. Even so we may never get to know what the immediately preceding conditions are, not at least till we know what life is. No more can the chemist get at the immediately preceding conditions of a chemical reaction, or the physicist at those of the transformations of energy, until the former knows what an atom is, or the latter understands the laws of molecular motion.

Lastly, in the so-called mental and moral sciences it cannot, unfortunately, be said that any but the most empirical generalizations have been made, owing to the difficulty of applying the test of experiment. Yet there is, indeed, some hope that experimental psychology, as a branch of the physiology of the brain, and the comparative study of psychical facts may prove fruitful in scientific results. But in spite of this disadvantage, it is none the less true that the states or acts of consciousness known as feeling, thought, and will are, like other phenomena, in all cases predetermined accurately by the mutual reactions of character and environment; character being built up on the one hand of

hereditary tendencies, and on the other of habits contracted by the repetition of similar actions. The subject-matter of psychology, therefore, in admitting of treatment along strictly deterministic lines, resembles that of all the other sciences. For it is as true of it as it is true of them that similar events are always predetermined by similar causes—a conception without which the whole fabric of human knowledge would crumble into chaos, and all intellectual effort be a profitless waste of time.

But, although the various sciences may justly hope to be able to explain their own proper facts in terms of their own ultimate conceptions, it is not so easy to say whether those who have set about the attempt to effect a transition from one science to another have much reason to be sanguine of success.

It is true that during the last half-century we have become more or less accustomed to look upon the universe as essentially a continuous process in time, during which events have become successively more and more complex. Such a view is summed up in the well-known definition of evolution as a change from an incoherent, indefinite homogeneity to a coherent, definite heterogeneity; and there is certainly some evidence for the truth of this conception. We know, for instance, that the more specialized forms of life, the higher animals and plants as we call them, are developments of a comparatively recent date; that these were preceded by simpler and more primitive types; and that earlier still was a period in which Life itself had not yet come into existence. Further, there may be, perhaps, some reason for believing that, in the remotely early history of this planet, inorganic phenomena were less complex than we now find them to be, having in fact since then undergone a physical and chemical differentiation from the primaeval terrestrial substance. But whether this be so or not, life at least had once a causal beginning, and the gap existing between the sciences of biology and of chemistry and physics is a breach not in the continuity of nature, but in that of human knowledge.

It may appear to some thinkers quite pertinent to urge that, for the practical purpose of making immediate progress, it is better, at least for the present, to keep the sciences apart. But, so long as any region of nature remains unexplored, it is illegitimate, and will be found impossible, to forbid it to any competent inquirer. Indeed, one highly trustworthy experimental chemist

has already offered some explanation of one of the functions of the living organism, as observed in the simplest forms ; while the assiduity with which the sciences of thermo- and electro-chemistry are being pursued tends perhaps to show that the hope has not been abandoned of establishing a connexion between other branches of knowledge. But whether the ultimate conceptions of the several sciences are reducible or not to a common conception ; whether or no life, chemical affinity, and molecular motions can be expressed in the same terms as are observed molar motions, while all phenomena become deducible from the laws of these : there yet remains for the complete philosopher the necessity of inquiring into what is involved in his ultimate conceptions of time, space, matter, motion, and the rest. Or, in the event of it being found impossible to bring these ultimate conceptions together under one, he must show clearly how they are all but different ways of looking at those abstractions which every man makes for his own particular purposes from the concrete facts of ordinary experience. Such an inquiry might fitly be styled ontology, the knowledge of being in its various forms. Thereupon for the materialist nothing more remains to be investigated. Ontology is identical with metaphysics, and the desired system of the universe is attained. But there is one fact which the materialist forgets to analyse, and materialism absolutely fails to explain, a fact which is indeed the hardest to understand of all, and that is knowledge ; and, with knowledge, those other facts of self-consciousness, feeling, and will. The philosopher who ignores this question has no claim to put forward his system as complete. Even though no metaphysical system is final but only an approximation towards, or, better, the expression of, an absolute system, still it is incumbent on us to attempt to form some, if it be but a very imperfect, theory of the universe. Otherwise we must be willing to acquiesce permanently in a position of philosophic doubt, and to make that which should be but the beginning the end of philosophy, namely, a scepticism, which indeed cannot be argued with, but will inevitably refute itself by doubting its own existence.

Assuming, what very few would be found to deny, the existence of knowledge, what is meant by it ? Knowledge is a relation between a subject, or knower, and an object, or known. But, says the physical realist, I am my nervous system : and knowledge means simply the sum total of the relations between my nervous

system and the external world. All that is meant, therefore, to take a concrete instance, when I say I know that trees are green, is that the external objects known as trees act upon, and are related to, my nervous system, in so far as regards that quality known as colour, in a certain definite manner. The nervous system retains these impressions in exactly the same way as the wax retains the impression of the seal, and thus memory, imagination, and so forth, are easily explained. According to this supposition the human being is nothing more than an exceedingly complicated machine, reacting variously to its environment—such a machine as one might perhaps hope to make by combining, say, a photographic camera with a phonograph and a thermometer. Moreover, they who uphold it must, if consistent, believe that each of these instruments possesses knowledge, and therewith, presumably, an illusion as to its own self-consciousness. For, on this view, self-consciousness, and the belief in a subjective as of a nature distinct from that of the objective, in a noumenal as opposed to the phenomenal, is the merest illusion. The universe is reducible to a number of series of objects coexisting in space and following one upon another in time ; the illusory ego being simply the merely objective fact that certain parts of the series are gathered together into what we commonly term the experiences of individual personalities.

Is such a theory tenable ? Can knowledge be explained in this way as the result of impressions falling, through a series of relationless atoms, upon the *tabula rasa* of a nervous system ? Is there no difference between human consciousness and the negative used to take a combination photograph, or the wax cylinder that registers the successive sounds in a concert-room ?

On the contrary, the nature of knowledge necessitates the belief that there must be something which unifies these atoms of sensation into an experience, beyond the mere objective fact that they are so unified. Again, this something, this knowing subject, must be itself outside the objects which it relates, and therefore is in no sense a product of, or co-ordinate with, them. For, if it were, it would thereby immediately lose its subjectivity and become merely objective. Further, knowledge being essentially a relation between a subject and an object, if it be said that it is impossible to arrive at any certainty with regard to the existence of the subject, it becomes thereby equally impossible to be certain

of the relation. Hence the materialist is forced to confess that, though he has a knowledge of objective phenomena, he does not, and never can, know that he has that knowledge. Materialistic writings, of course, are full of such phrases as idea, conception, law, and the like. These imply a certainty in regard to the existence of knowledge, and by their use the self-contradiction involved in the materialistic attitude stands self-condemned.

Knowledge, then, instead of being a relation between purely objective phenomena, is a relation between me, a subject, and certain objects; this relation being variously termed sensation, perception, or conception, and its seat or necessary condition being my nervous system. And, let us be careful to remember, we never can, in our knowledge of phenomena, get beyond this relation. 'I know such and such a thing' means, therefore, that I am related to, in unifying, certain objects of sensation, perception, or conception. These unified data of experience I term objective phenomena. Myself who unify them I term subjective, noumenon. These phenomena, including psychical facts, which are the successive acts of unification performed by the knowing subject, appear to me under the forms of space and time; and they bear objective relations to one another, which relations it is the function of the sciences to investigate.

My own and other bodies, together with their nervous systems, are among these phenomena; and among these relations are those between what is commonly called the external world and the nervous systems of other bodies, and, by inference, of my own. 'The baby new to earth and sky' is serenely unconscious that he has a body, much more a nervous system. And we might, conceivably, remain permanently in this condition; though it would not, therefore, follow that we should be devoid of knowledge.

My body only means a particular set of phenomena of which I am nearly always conscious, and my nervous system a set of phenomena which, as scientific investigation has taught me, conditions, that is, is the invariable accompaniment of, my having any knowledge at all.

The appearance of these phenomena under the form of space leads us to confuse 'I' with 'my body'. Again, it leads us to imagine that 'I' am spatially located in my brain. Impressions come from external objects to the brain, and then get in some

mysterious way to 'me', who retain them in my memory as ideas. It is this futile tendency, even when the purely materialistic hypothesis has been discarded, to persist in endeavouring to find a medium between consciousness and the external world, still regarded as endued with a reality of its own, that makes the path easy for those who confound the objective relation of one phenomenon to another, of my body to other objects, with that subject-object relation which is the very essence of knowledge.

And now let us inquire a little more deeply into the nature of this relation. It was the great merit of Kant to have insisted as against the passive-substance theories of Locke and Berkeley, which Hume drove to their logical conclusion in a sensational atomism, that alike in sensation—for sensation is a critical faculty—in perception, and in conception the subject is active; unifying the unrelated data of experience; imposing relations on these according to certain laws of its operation, that is to say, in space and time; and so attaining to a certainty of its own existence, though never being the final term in the series which it unifies.

What, then, of the object of knowledge—that which, having been received in experience, the mind informs?

We have already seen the fallacy of supposing that external objects, having an existence of their own in space, produce impressions on the nervous system which in some inconceivable way become transformed into non-spatial sensations and ideas. And it is a cognate error which holds that there remains over, after we have stripped the object of knowledge of its admittedly ideal attributes, a residuum, an ultimate cause of sensation, unknown and unknowable, but clothed with a separate reality of its own—a thing-in-itself—which impinges upon us in experience and causes in us a knowledge of its attributes, so that there is between mind and matter an intermediate world of unreal ideas.

For it is impossible, upon this dualistic hypothesis, to understand how that which is so far out of relation as to be unknowable can so far come into relation as to give a manifestation of itself, and have its matter subjected to the informing operations of mind. On the contrary, the object of knowledge is nothing but the sum of its own attributes and relations. It is not unknowable things-in-themselves, producing in us unreal ideas, that constitute objective reality, but the knowable and nothing else. The real is that which can be synthesized, an objective relation which can

and must be itself related to some subject. Only existing therefore as the knowable, the real is also necessarily ideal. Thus the distinction between perception and conception, between impressions and ideas—erroneously regarded as respectively real and unreal—comes simply to this. The individual object of perception is only the meeting-point of universal conceptions, *plus* the here and the now ; which latter resolve themselves into relations of coexistence with and succession to other objects. Perception, in perceiving the particular, to adopt Aristotle's famous simile, arrests one universal, round which the rest rally like fleeing troops on a battlefield ; while in conception the mind grasps some only of these universals. The objects of both are alike ideal, and they are real in the same sense of the word. The real is the ideal, and the ideal is the real. The material, objective, phenomenal universe is to be expressed only in terms of the subjective and noumenal, that is, of self-consciousness. Not that it becomes therefore any the less objective. But it is impossible to explain the subjective in terms of the objective ; and, if, as Hegel put it, dualism is philosophic death, no other alternative is left but to say that the phenomena which constitute the object of knowledge are the creation of the mind itself.

It still remains for us to inquire *what* mind ? Are we to say with the solipsist, *Alles ist ich, ich bin alles* : and to call all that comes to us in experience equally real ; to ignore all other criteria of reality but this ; and, in fact, to obliterate the distinction between reality and unreality, and almost that between self and not-self ? Or, rather, must we not remember that the data of experience are independent of us ; and accept the inference, drawn from the existence of other bodies similarly constituted, and having similar relations in time and space to our own, that personalities other than ourselves but knowing the same universe as our own exist—an assumption, indeed, without which we could never give a consistent account of our own knowledge at all ? Hence, must we not believe in a difference between reality and unreality, such as lies at the bottom of what is, after all, common belief ? For, to the ordinary man, objective reality means that which is coherent with either his own or other people's experience. To the scientific man, the real means that which is intelligible to himself and others—that which falls into its place in an organized system. To the moralist, the real is the rational and the rational

the real, a sense which includes the two others. Ultimately, then, the question, Is a thing real ? must mean, Is it related absolutely as it seems to me to be related ? And the further question, What is that whereto that which is an absolute relation is related ? compels us to acknowledge an absolute mind. The object of such a mind is, not a world of ideas midway between subject and thing-in-itself, the phenomena of Kant's noumena, the knowable manifestations of Herbert Spencer's unknowable. Reality, facts, a world of ideal relations, not self-existent entities, or unrelated atoms, but an organically connected whole of thought completely thought out—herein consists the object of knowledge to an absolute mind. Whether such a mind could be spoken of as personal is an open question, which it is hardly necessary here to discuss. But it may be observed, firstly, that the idea of personality, which gains in force from the mischievous confusion between body and person, seems to be inseparably bound up with the moral and intellectual imperfections of human individuals ; and, secondly, that this mind must be regarded not so much as something outside individual minds, differing from them in capacity, yet related to them as they are to each other, but rather as an inner harmonizing activity between them, of which they are, so to speak, the expressions, and in which they are included.

To such an idealism it is often objected that it is as true to say that nature makes the understanding, as that the understanding makes nature—that the world makes God, as that God makes the world. This, indeed, is the dualistic position in which Kant leaves us, the subject which gives the form not being more necessary for knowledge than the object which supplies the matter. But, quite apart from the inconceivability of the existence, outside thought, of that metaphysical phantom the thing-in-itself, in knowledge the subject, not being derivable from the object, is the more important. For, logically, that is, so far as validity is concerned, the active is prior to the passive, as form or εἶδος to ὕλη or matter. Hence the ὕστερον πρότερον of materialism, which tries to explain mind, the subject, by matter and motion, the object. It starts with the most elementary categories of being and not-being in order to evolve the universe therefrom, instead of beginning, as Hegel does, with self-consciousness, the highest category of all, and, let it be added, the most fully knowable. For, since knowledge is essentially a relation, where this knowledge

is most intimate, it is most transparent; where subject and object are one, there and there only is knowledge complete.

Now it appears at first sight that there still remain obstacles which the doctrine of evolution offers to the acceptance of such a system as this; and the nature of these obstacles is twofold. Firstly, there is the metaphysical difficulty: evolution presents to us the objective universe as essentially a continuous process in time. All its events are strictly and absolutely determined by preceding events. Its culminating point is the genesis of consciousness, which, therefore, it would appear, having undergone a development in time, is the product of the non-conscious. It is the doctrine of becoming, not of being. According to idealism, on the other hand, that ultimate reality which is found in self-consciousness is essentially timeless. Idealism can explain being in terms of knowing, but it seems as if it could not explain becoming. Secondly, we have what may be called the moral difficulty. Evolution puts before us 'Nature red in tooth and claw with ravin'—nothing but a selfish struggle for existence, with many imperfections, many degradations, and all its small advance at a tremendous cost of individual life. This of course is the problem of evil; and its solution, although very largely a question of temperament—for a pessimist will be a pessimist to the end of his days—also depends on the meaning attached to the word 'perfection', which can itself only be satisfactorily thought out by a consideration of what is involved in the idea of time.

In the first place it must be insisted that, with regard to the development of consciousness, no intuitionist theories are allowable which invoke cataclysms to explain what is explicable by known laws—which talk of spiritual influxes coming no one knows whence or how, and arbitrarily and inexplicably descending upon the brain of the greater ape, and there developing into thought, feeling, and will. We must acknowledge the development of consciousness in time, of mind with body, if we accept the fact of evolution at all. The stages that we imagine the race to have passed through are paralleled in miniature in the embryogeny of each individual. If, therefore, what is true of the individual is also true of the race, and if in the growth of the embryo from the germ—and this resembles exactly in all essentials of structure so lowly organized a creature as an amoeba, which forms the starting-point for the evolution of the whole animal kingdom—

we can fix no point at all, draw no hard and fast line, for the first appearance of consciousness ; then it is impossible to believe otherwise than that the rudiments of consciousness have been present, even as they are undoubtedly present in their now living allies, in those lower forms from which the human race has been derived.

And yet mind does not evolve from what may be called, for the sake of convenience, matter ; nor is mind a product of physical forces, nor a higher form of life. The psychical is not the physical, but an aspect of it. Just as new properties come into being when oxygen and hydrogen unite to form water—properties which could not have been foretold from the known peculiarities of the elements, though the molecular motions might doubtless have been predicted from the movements of the atoms ; or just as blue colour appears when particles vibrate with a particular periodicity, though the vibrations are not blue : so also does the psychical arise when the physical has attained a certain degree of complexity, and with the further development of the one series the other marches side by side.

Now, if the view that has been taken above be true—if it be the understanding that makes nature—then time must be looked upon, not as the self-existent succession of things-in-themselves, but as a law of the intelligence—a form, to use Kant's expression, under which the mind, itself out of time, sees the phenomena which it makes. Mind is not here and now, or then and there, but simply is. If it appear to evolve in time, it is because time is the form under which it sees the phenomena on the progressive knowledge of which this development depends. To object, therefore, that in this case, say, the palaeozoic fauna can never have existed, because there was then no mind for which they could exist, or, in the same way, that only those parts of space exist which are being actually perceived, is entirely beside the point. For all that is meant by saying that certain things existed at a certain time, or in a certain place, is that they appear to the mind in a certain order of precedence and succession, or in one of coexistence, with regard to other things. Since this order is not, any more than the things, self-existent, it makes no difference whether the mind, for which alone it and they exist, has reached any particular stage of development or not. They do not exist for us who are now and here, but they existed or will exist then

and there for us. Mind, in short, is not in time, but time is in mind. And, lastly, if this were not so, if time were ultimately self-existent, then—and the same reasoning applies to space—either there would necessarily be an infinite regress and progress of events in time; or there would be a period of actual finite time in which events happen, preceded, and to be succeeded by, infinite periods of possible time in which events might but do not happen: both of which suppositions seem unthinkable. On all grounds, therefore, we are compelled to call time a form of our perception. The ultimate explanation of existence is thus to be found not in a temporal *prius* out of which all things emerge, but in a logical *prius* which they presuppose, and towards which temporally they seem to move.

What is to us a time-process from the less perfect to the more perfect, what is to us the development of the notion from pure being to the absolute idea, is really not construction but reconstruction. That which, to use Aristotle's language, is *χρόνῳ πρότερον*, or earlier in time, is *λόγῳ ὕστερον*, or later in importance. Metaphysics must explain existence, not aetiologically, as science does and *must* explain its phenomena, but teleologically—in terms, that is, not of its origin but of its validity, or, in other words, of its final cause; because everything is not merely that which it sprang from, nor even that which it seems to be, but also that which it may become, though we cannot say of it 'it is' till it has become. Following Hegel, then, we must rationalize the universe, not by supposing that which is the presupposition of the lower categories to have developed in time out of them, but by explaining it in terms of the highest category, self-consciousness; which, in seeking the objective as that through which alone it can realize itself, compels itself to look upon the universe of its own creation under the forms of time and space.

Let us turn to the second difficulty. It is impossible to attempt the solution of the problem of evil—as impossible as we have found it so to explain knowledge—by a dualistic reference to the unmanageable *ὕλη* which the *δημιοῦργος* or divine artificer never quite succeeds in reducing to shape.

Evil and good are essentially relative terms. Evil, according to the conception of the evolutionist, means failure on the part of the organism to adapt itself to a new environment, provided that in so doing it also fails to progress. For it is not true, as

has been sometimes asserted, that adaptation to environment, the mere persistence of an organism, is the only test of excellence ; and that, therefore, to quote a well-known example, the tadpole which becomes a degenerate ascidian is as 'good' as that which has gone on developing till it has reached the man. On the contrary, it is possible to speak in a perfectly scientific, that is, aetiological, way of the persistence, or non-persistence, of the development or degeneration, of the specialization or reversion, of an organism as facts due to adaptation, variability, the struggle for existence, and so on ; and yet also to speak of a higher, or better, organism as one that has attained, by adapting its own variability to an ever-changing environment, to an increased and increasing division of physiological labour. At the same time, it must be freely admitted that the word 'good' or 'higher', as used by the evolutionist in this sense, is one borrowed in the first instance from the terminology of ethics, and used there to describe the development of the moral character.

Now this differentiation must involve a greater limitation by one another of the different organs in the organism ; and so it is that any one organ has in a higher stage far less scope for individual exercise than in a lower. Hence, what once contributed to survival may now, if indulged in to the same degree, cause pain, failure, sin, or whatever we choose to call it. 'For man when he has climbed to the top of the ladder kicks over that by which he rose, and brands with the name of sin those qualities once good, now bad, which enabled him to be what he is.' It is therefore impossible to speak of any action, or quality, as being in itself either good or bad ; and quite futile therefore to suppose that perfection is 'some far off, divine event to which the whole creation moves'. If perfection, or unconditional good, exist at all, it is to be found here and now, if we could only see it. And, it is just in the process itself that it consists, namely, in the effort, at the expense of necessary error and pain, towards self-realization ; which, be it added, is the only ultimate criterion, as it is the only ultimate end, of conduct.

Now, in that particular form of evolution which it is the function of ethics to study, we find, as a matter of fact, that all advance, in the sense above indicated, is accompanied by a continually increasing control over the environment—that is to say, by the development of the will. Will is thought intensified, the

momentary determination by the organic consciousness of its own environment. Looked at as part of the time-series, these acts are conditioned by one another, and by the environment, in the same way as knowledge of an object is conditioned by previous knowledge and by the object itself; and, if time were ultimately self-existent, the will would not, and never could, be completely free. But, just as thought is logically prior to, and the presupposition of, that out of which it seems to develop, so will—though conditioned in time by its own previous acts, as a function of that self-consciousness which is realizing itself in the irrational order of time and space which it makes for itself—is free. For the development of the will accompanies the advance of knowledge. Where the organism is a self-conscious subject, survival, or moral equilibrium, implies knowledge; while failure, or the loss of moral equilibrium, means ignorance. Thus it is that true virtue is always μετὰ λόγου, because it implies control over any possible environment. Perfect freedom would be perfect understanding.

If these considerations be just, then the gigantic time-process in which science has found the objective, phenomenal universe to consist, in which all events are rigorously predetermined by others, and during which the evolution of conscious beings has taken place, can only receive its full and ultimate meaning thus: namely, when it is shown, by an inquiry into the credentials of science—in other words, into what is involved in the fact of knowledge—that, outside phenomena, there is something else to which they owe their none the less real existence, namely, a noumenal or subjective. Such an inquiry may fitly be termed philosophy *par excellence*, or metaphysics. Now where does the system of metaphysics that has here been outlined, whether true or false, seek to find the ultimate essence of existence? Not in a crudely anthropomorphic God, as conceived, unfortunately, by too many religions. Nor again in that objective force, the far away first cause of all things that are, which others have vainly sought to deify. It seeks to find it in that subjective consciousness, that union of thought, feeling, and will, the elements of which each apparently separate individual may find hidden in his own soul. Different though the aims and methods of the man of science and the philosopher appear, still each has much in common with the other. For a faith in the ultimate rationality of the universe is

the presupposition of all scientific endeavour and of all speculative effort ; and the philosopher and the man of science alike are content to

> Strive and hold cheap the strain,
> Learn nor account the pang,
> Dare, never grudge the throe.

Herein they find that *εὐδαιμονία* or 'blessedness' which, as one of the greatest of philosophers if not also of scientists would have told us, is the *τέλος τέλειον*, the end in and for itself, of all human aspiration whatsoever.

# A SKETCH OF THE HISTORY OF PALAEOBOTANY

## WITH SPECIAL REFERENCE TO THE FOSSIL FLORA OF THE BRITISH COAL MEASURES

By E. A. Newell Arber[1]



### Introduction

The history of a particular Science depends in no small degree on certain events, which may be more or less peculiar to that branch of knowledge, or common only to a series of related studies. In the case of Palaeobotany, as of Palaeozoology, the factor which more than any other has controlled and still continues to dominate its progress is, literally, the unearthing of documents which are still legible. These ‘ sources ’ (i.e. the fossils themselves) have first to be found and then removed from Mother Earth. In this respect Palaeobotany presents a great contrast to the Experimental Sciences. We are absolutely dependent on what we have dug out, and since, speaking generally, it is very far from easy actually to locate the evidence in a series of otherwise barren rocks, our progress depends largely on a succession of happy chances occurring often only at long intervals. The cherished belief of to-day in regard to some particular point in fossil botany may have to be shamefacedly abandoned to-morrow, simply because something more complete which demands a revision of the received opinion, has been literally turned up.

No doubt in the history of this, as of other sciences, there is a hinterland in the form of a prehistoric period, so-called. Among the curios known and commented on from quite early times down to the mediaeval period, there were some which happen to be fossil plants. Perhaps the most celebrated of these cases is that

[1] Owing to the death of the author before this paper was revised, the responsibility of editing it has fallen to his wife.

# PLATE XLVIII

Fig. 1. *Cycadeoidea etrusca*, Cap. and Solms, Pl. I, Fig. 2 of Capellini, G., and Solms-Laubach, H., *I Tronchi di Bennettee dei Musei Italiani*, Mem. R. Accad. d. Scienze di Bologna, Ser. 5, T. II., 1892.

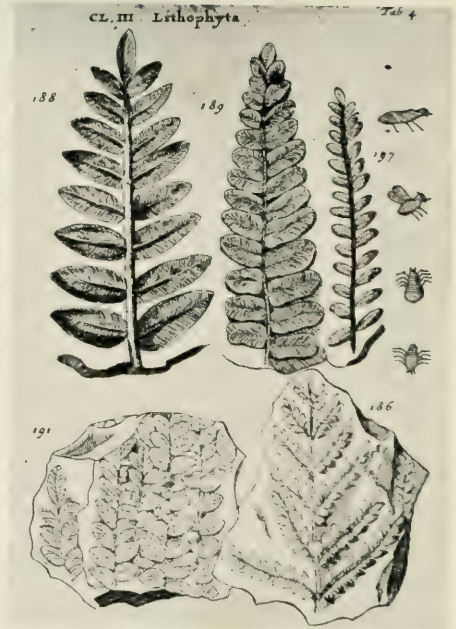



Fig. 2. Edward Llhuyd, *Lithophylacii Britannici Ichnographia*, 1699, Pl. IV (reduced). 186, *Lithopteris*; 188, *Lithosmunda*; 189, *Lithosmunda minor*; 191 and 197, *Trichomanes*.

of the silicified stem of a species of *Bennettites* (plants dimly related to the modern Cycads), which was no doubt unearthed from Jurassic rocks in Italy something like three thousand years ago (Plate XLVIII, Fig. 1). At any rate, this particular fossil was known to the Etruscans who about 1000 B. C. inhabited the district where Marzabotto now stands. Very probably it was the only example any of them had ever seen; at any rate they appear to have regarded it as an object of superstitious reverence and great rarity, since on the occasion of the death of some important member of their community, it was placed in his tomb in addition to other offerings indicating respect. However, to re-bury a fossil is but to preserve it anew for posterity, for some years ago this particular specimen was dug up again and formed the subject of brilliant botanical researches by a celebrated German professor. Further, upon it is founded our present knowledge of the structure of the cones of this particular plant, constituting one of the most astonishing advances ever made in fossil botany. The present resting-place of this specimen is a museum at Bologna.

Again, every one is aware that the ancient Phoenicians knew of Amber, which is part of a fossil plant. Jet, another vegetable fossil, was also familiar to the ancients. Many other instances might be given, the written history of which begins in classical or mediaeval times. But in this brief sketch it is not proposed to explore this hinterland further. The real history of the science, as a science, dates only from the Renaissance, and to reckon even from that period to the present day involves the inclusion of a lengthy early stage which, strictly speaking, was Pre-scientific.

### The Pre-scientific Period

In the earliest stage in the printed history of our science, such fossil plants as found their way into a collector's 'cabinet' were mere curiosities. It was not admitted—in fact it was universally denied—that they were relics of plants which had actually lived on the surface of the earth in a bygone age. Geology as a science did not exist then, nor indeed until after the Renaissance. Fossil plants were mere sports of nature (*lusus naturae*). Curiously enough such things actually occur not infrequently, cases where purely mineral concretions simulate plant remains in shape and form in an extraordinary degree being not uncommon in the older rocks. It was therefore not unnatural to regard all such cases as

obvious frauds, and the interest in them lay chiefly in the curiosity aroused by the completeness of the delusion. At the same time, despite this fact, the science was advancing under the influence of the 'cabinet' and its curios.

The birth of the Renaissance in Italy shed new light on the origin of true fossils, and that light travelled quickly. The Italians, among them Leonardo da Vinci, realized that many of the contents of the cabinets were really of organic origin, and, once this was admitted, an added interest naturally attached to them. They were no longer mere curios. The first intellectual stimulus had arrived. Attention became focussed on them, speculation became rife, and as the result geology through palaeontology was born. At the same time it must not be imagined that palaeobotany had by now become a science, using that term in the modern sense. It had not. It had been born and was on its way to become a science. So far that was all, and for many years the study remained pre-scientific, rather than scientific.

And here we may pause to note the change induced in the terminology of the contents of the cabinet by the more exact knowledge which was slowly becoming evolved. The term 'fossils' originally meant anything dug up out of the earth. Sands, clays, marbles, &c., were as much fossils as anything else delved out of the earth's crust. With the coming of the scientific renaissance, a fresh distinction in terms was inevitable. *Real* or *natural* fossils were distinguished from what were called *extraneous* fossils. The former were what we should now describe as lithological, petrological, and mineralogical specimens. 'Extraneous fossils', to-day universally known simply as 'fossils', were the relics of what originally had been organized bodies, either animal or vegetable. The growth of collections and the dawning realization of these and other distinctions, seem to have led to the preparation of manuscript lists, enumerating the contents of the cabinets.

The next great step was obviously the printing and publication of such catalogues, including not only the names which the owner gave to the objects, but descriptions of their characters. These catalogues—which of course at first included every mineral object regarded as possessing any interest at all—in so far as they related to fossil plants, represent the sum total of the palaeobotany of the period. The earliest of these catalogues which contains any reference to British fossil plants was published in 1699 by Edward

Fig. 3. One of the original cabinets belonging to John Woodward (1665-1728), whose geological collections form the nucleus of the Sedgwick Museum, Cambridge. On the open front a volume of Woodward's MS. Catalogue is shown, and four of his fossil plants.

Llhuyd (=Lhuyd=Luidius) (1660–1709), who was keeper of the Ashmolean at Oxford. This work is termed *Lithophylacii Britannici Ichnographia*, and a second edition was published in 1760 after Llhuyd's death. This catalogue was illustrated, and several Coal Measure plants are represented by fair figures. They were, however, like all the others catalogued during this pre-scientific period, nameless, by which is implied that they were not referred to genera and species, a much later device dating from the time of Linnaeus. For instance, Fig. 188 on Plate 4 of the first edition (Plate XLVIII, Fig. 2), which is probably intended to represent *Alethopteris Serli* (Brongn.), is described as 'Filix florida Mineralis sive *LITHOSMUNDA* Cambrobritannica. E fodinis Glamorganensibus superiùs dictis'. Again, Fig. 202 on Plate 5, probably *Annularia sphenophylloides*, is described as '*RUBEOLA* mineralis. E fodinis Actonensibus'.

EDWARD LLHUYD (1660–1709)
From an initial vignette in the Register of Benefactors, Ashmolean Museum, 1708.

The specimens figured by Llhuyd were mostly derived from coal-mines in Glamorganshire and Somerset and the Forest of Dean, and a few from Denbighshire and Flintshire.

After the publication of Llhuyd's work, the collection of fossil plants received an immense additional mental stimulus from the initiation of the great controversy on the *Flood Theory*, towards the end of the seventeenth century. It was then held that there had been one great deluge, the flood of Noah, and that all fossils were the productions of this catastrophe. Clearly, then, the explanation of the origin of all fossil plants was to be sought for in the early chapters of Genesis. They were all of precisely the same geological age, and all had been entombed in water-borne sediments during a comparatively brief period. These contentions are fully set forth in the next great handlist—*A Catalogue of English Fossils* (1728–9), by Dr. John Woodward.

Woodward (1665–1728) was a medical man, and a Professor at Gresham College, London. By his will, he left his large collections, to which the catalogue in question relates, to the University of Cambridge, and at the same time founded the Woodwardian

Chair of Geology in that University. This collection was contained in several cabinets, and these are carefully preserved to this day in the Sedgwick Museum of the University in exactly the condition in which they were left by Woodward (Plate XLIX, Fig. 3). The Woodwardian collections are undoubtedly the oldest still in existence, and in them one can actually see 'Cabinets' as they stood at the end of the eighteenth century. Further, it is these very cabinets which have formed the foci round which the present enormous and valuable geolcgical collections belonging to the University have crystallized out, so to speak, in the last two centuries.

Woodward, in common with the celebrated Swiss geologist, Scheuchzer, was one of the great Apostles of the Flood Theory. In the preface to an earlier work on this hypothesis (*Essay toward a Natural History of the Earth*, 1695) he says :

'It will perhaps at first sight seem very strange, and almost shock an ordinary *Reader* to find me asserting, as I do, that the whole Terrestrial Globe was taken all to pieces and dissolved at the Deluge, the Particles of Stone, Marble, and all other solid Fossils dissevered, taken up into the Water, and there sustained together with Sea-shells and other Animal and Vegetable Bodies ; and that the present Earth consists, and was formed out of that promiscuous Mass of Sand, Earth, Shells, and the rest, falling down again, and subsiding from the Water. . . . The other Instance I make choice of shall be of the Universality of the Deluge.'

In Woodward's catalogue of English Fossils (*An Attempt Towards a Natural History of the Fossils of England*, &c.) the theory of the Deluge was pushed to extremes, and efforts were made to fix the season during which it took place. In the second volume (1728) of this work (p. 59) we read :

'The Hazle Nuts, digg'd up in *England*, are rarely such as appear to be ripen'd. The Pine Cone are in their vernal State ; as are all the Vegetables, and the young Shells. The Deluge came on, and a stop was put to their further Growth, at the End of *May*.'

Again, in the first volume (p. 21), which appeared a year later, we find the following remarks :

'Of all the Fossil-Nuts I have ever seen, either in the *North*, the *Isle of Wight*, or any other Part of *England*, tho' some few, perhaps by reason of some particular Advantage of Situation and

# PLATE L


Fig. 4. David Ure, *The History of Rutherglen and East-Kilbride*, Pl. X (reduced).

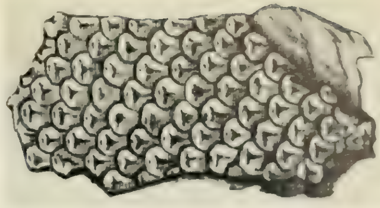


Fig. 5. E. M. da Costa, Phil. Trans. 1757, Vol. 50, an impression 'from a coal-pit in Yorkshire'.

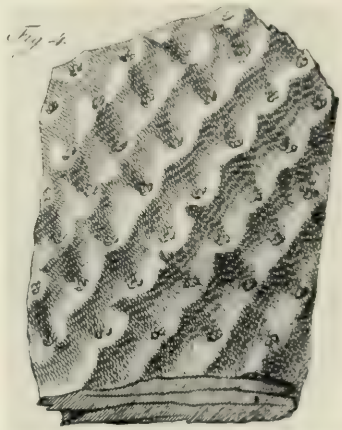


Fig. 6. E. F. Schlotheim, *Die Petrefactenkunde*, 1820, Pl. XV, Fig. 4, *Palmacites*.

Fig. 7. W. Martin, *Petrificata Derbiensia*, 1809, Pl. X (reduced), *Phytolithus Filicites* (*striatus*) = *Alethopteris lonchitica* (Schl.)

Fig. 8. W. Martin, *Petrificata Derbiensia*, 1809, Pl. XI (reduced). *Phytolithus Plantites* (*verrucosus*) = *Stigmaria ficoides*, Brongn.

Sun, are somewhat larger ; yet the generality of them appear to be of about the Growth and Condition that Hazel-Nuts usually are at the end of *May* or the beginning of *June* ; and that the Deluge began at that time of the Year.'

The dogmas of the Diluvialists long continued to attract attention to fossil plants. In 1758, for instance, James Parsons (1705–70), in a paper published by the Royal Society (*Philosophical Transactions*, vol. 50. pt. 1 for 1757, p. 396), on Eocene fossil fruits from the Isle of Sheppey, disputed Woodward's conclusion that the Noachian deluge took place in May, since, according to Parsons' specimens, ' there are the stones of fruits, found fossil, so perfect as to make one imagine they were very ripe, . . . which would induce one to think that the deluge happened nearer Autumn '. (p. 402.)

Apart from catalogues of cabinets, other records of fossil plants began to appear in the eighteenth century in the accounts which were published of the history, antiquities, and especially the curiosities, of particular regions, districts, or places.

For instance, in Robert Plot's (1640–96) *Natural History of Oxford-shire,* which appeared in 1677, a Stigmarian rhizophore (Plate III, fig. 11) is figured, which Plot concluded (p. 98) ' seems ' to represent a *Carp* or *Barbel,* the best of any Fish which I have yet compared it with '. Another work, nearly a century later, David Ure's *History of Rutherglen and East-Kilbride,* published in Glasgow in 1793, contains the earliest illustrations of fossil plants from Scotland (Plate L, Fig. 4). No less than four plates (X–XIII) of very fair figures, considering the period, are devoted to them, and they are also closely compared with living genera. The earliest British *memoir* (as opposed to catalogue or guide book) solely concerned with Carboniferous fossil plants was published in 1758 in the *Philosophical Transactions* of the Royal Society of London (vol. 50, pt. i for 1757, p. 228, Pl. v). This is the prototype of a vast series of subsequent papers, many of them having appeared in the same publication. The author of this paper, Emanuel Mendes da Costa (1717–91), entitled it ' An Account of the Impressions of Plants on the Slates of Coals '. It describes briefly specimens of *Sigillaria, Stigmaria* and other genera from various coalfields (Plate L, Fig. 5). The occurrence of such fossils in coal-bearing rocks had previously been put on record by Woodward and several foreign observers, but in Da Costa's paper an attempt

is made to examine and discuss these fossils themselves with more thoroughness than is found in earlier writings.

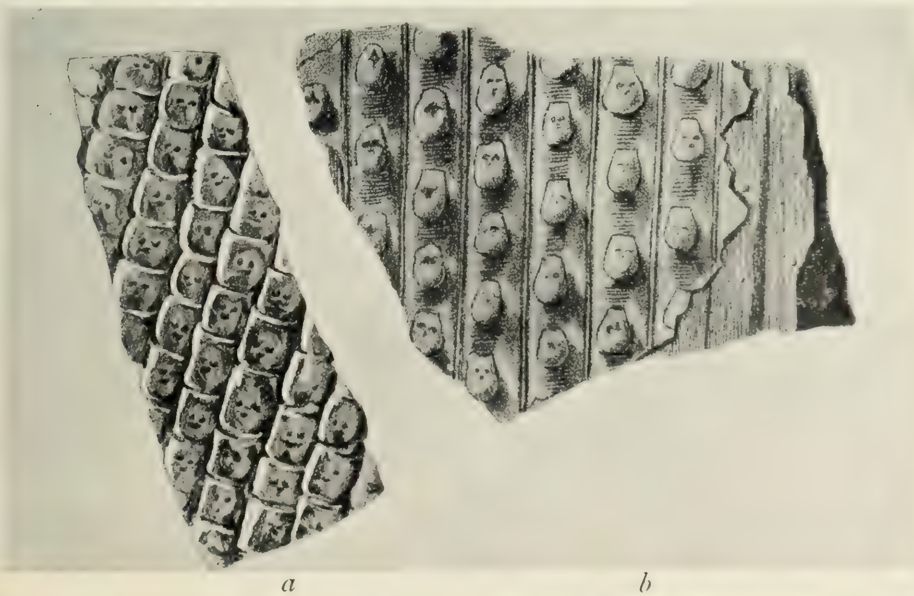
However, about the beginning of the nineteenth century new ideas began slowly to prevail. It was seen that if any real progress was to be made with the study of fossil plants it was necessary not only to describe them, but to indicate them binomially (i.e. to refer them to definite genera and species) on the system which Linnaeus had applied some years previously to living plants and animals. Further, a somewhat later period witnessed the final overthrow of the Flood Theory and the birth of stratigraphical geology. It came to be recognized that plant-bearing rocks were not all of the same geological age, but belonged to different periods in the history of the earth's crust, as is witnessed by the diversity of fossil floras which they contain. It is, of course, impossible here to trace the rise and progress of this great biological conception, but it may be noticed that, once its truth was admitted, the necessity for the binomial usage was felt to be even more pressing than before. Thus the systematic side of the subject received a great impetus.

The binomial system of nomenclature, as applied to fossils, did not originate in this country. It must regretfully be admitted that British workers played no part at its birth. It was only in quite modern times that British contributions began to exert any profound influence on the study of fossil plants.

### The Transition to the Scientific Period

The Scientific Period begins with the adoption of the binomial system and the foundation of systematic stratigraphy. The transition period is beautifully seen in the works of a single writer, of whose memoirs only two contain any reference to fossil plants. This was a German, Ernst Friedrich, Baron von Schlotheim (1764–1830), of Gotha. Schlotheim in 1804 published his *Beschreibung merkwürdiger Kräuter-Abdrücke und Pflanzen-Versteinerungen.* In this book he gives good figures of a number of Coal Measure plants, but no names were applied to them. This book belongs to the pre-scientific period. But in 1820, sixteen years later, he published his *Petrefactenkunde auf ihrem jetzigen Standpunkte durch die Beschreibung seiner Sammlung versteinerter und fossiler Überreste des Thier- und Pflanzenreichs der Vorwelt erläutert*, in which the plants figured in the first memoir were named binomially. Plate L,

Fig. 9. James Parkinson, *Organic Remains of a Former World*, 1804, Vol. I, Pl. V (reduced).



*a* *b*

Fig. 10. H. Steinhauer, Trans. Amer. Phil. Soc. 1818. *a*, *Phytolithus tessellatus* = *Sigillaria*

Fig. 6, shows one of the illustrations from this work. Since no one in the interval between the appearance of these two volumes had applied other names to the same fossils, the names in question still stand, but the interval of sixteen years cost Schlotheim the honour of being actually the first to apply the binomial system to fossil plants. The credit for this deed was gained by Steinhauer in 1818 (see p. 481). Before, however, we pass to consider the earliest stage in the Scientific History of Palaeobotany, there are other works belonging to the transition period to which a passing reference should be made.

In 1809 William Martin (1767–1810) published at Wigan his completed *Petrificata Derbiensia.* The earlier parts of this work had in reality been obtainable some years previously. The first four numbers were published in 1793, and were called *Figures and Descriptions of Petrifactions collected in Derbyshire.* The object of the work was to furnish 'coloured figures of extraneous fossils'. The illustrations, which included a few fossil plants, give very fair, though not really accurate, representations of the specimens themselves. Let us see, however, what the descriptions of the fossils were like. We will take two examples. What we now call *Alethopteris lonchitica* (Schl.), Plate 10 (reproduced in Plate L, Fig. 7), is described as '*PHYTOLITHUS FILICITES* (*striatus*) fronde bipinnatâ: pinnis ovato-oblongis' &c.; and *Stigmaria ficoides,* Plate 11 (reproduced in Plate L, Fig. 8), as '*PHYTOLITHUS PLANTITES* (*verrucosus*) trunco subcylindrico subramosa' &c. Here we see some primitive attempt at a generic name, but the distinction between the name and the diagnosis hardly exists.

In 1804 the first of three volumes of James Parkinson's *Organic Remains of a Former World* appeared, a most remarkable work considering the state of knowledge of that time. Parkinson (?–1824) was a medical man in practice at Hoxton, then *near* London. The sub-title of this work was 'an examination of the mineralized remains of the Vegetables and Animals of the Antediluvian World; generally termed Extraneous Fossils'. Here we see two fundamental ideas of the period clearly exhibited—firstly, that extraneous fossils were replicas of plants and animals which *had* once lived, and secondly, that all such fossils were nearly contemporaneous and dated from just before the Flood, which itself was the cause of the *present* existence of fossils. These conceptions marked a great advance on those of Llhuyd, who was ignorant of the real

nature of the objects which he described. Parkinson, however, fully understood how fossil plants had come to be preserved, and he carefully distinguished between impressions, casts, and petrifactions. The variety of petrified woods known to Parkinson was remarkable. Among others he describes or mentions the Tertiary wood of Loch Neagh in Ireland, the Cretaceous Palm and other woods of Egypt, the Forest layers in Peat bogs, the Oligocene lignites of Bovey Tracey in Devonshire, and the Permian Star stones (Psaronius) of Chemnitz, Saxony. He also discusses jet, leaves buried in amber, and plant-bearing clay-ironstone nodules, as well as shale impressions. He comments on the 'great difficulty of ascertaining even the genera of the plants which are thus preserved . . . dorsiferous plants and cacti most common', and on 'fossil flowers—their existence doubtful'; 'the tender and almost succulent substance of the petals, stamina, and pistilla, will furnish very little reason for supposing that they should resist a destructive resolution, sufficiently long to allow them to pass through those chemical changes, by which such a length of duration would be given to their original forms, as would secure their passing unchanged, in their figure, from the vegetable to the mineral kingdom'. Parkinson figured certain Coal Measure plants both from shale and clay-ironstone impressions, as well as several types of fossil wood, some of them from polished slabs, but not, it is important to notice, from microscopic sections. An example of his illustrations is shown in Plate LI, Fig. 9.

## The Scientific Period

The Scientific period of fossil botany dates from the year 1818, when Steinhauer first described binomially certain British Coal Measure plants in a memoir published in America. In the century which has since elapsed we can distinguish two fairly well-marked phases in the scientific study of fossil plants, one, which we may term the Pioneer period, extending from about 1818 to 1870, and a later Modern period from about 1870 to the present day.

### (i) *The Pioneer Stage*

The work of the Pioneer stage was essentially the collection and collation of evidence. The fossil-bearing rocks of all geological ages and in all civilized lands were studied. This work was originally and essentially palaeontological, and was chiefly carried

out by those whose inclinations were on the whole geological rather than botanical. Some were, it is true, professed botanists, and a few even great botanists, but the majority of the earlier workers on fossil plants, while by no means ignorant of the knowledge of the day as regards living plants, were professedly palaeontologists or connected with geological institutions. It is only in the Modern Period that the study has attracted a majority of strictly botanical workers, though even now some of the best-known names are those of palaeontologists. Similarly, with few exceptions, the fossils collected during the last century have found a home with other fossils, i.e. in the geological rather than the botanical museums of the world.

But from whichever side the subject may have been approached the result was the same. A flora from one particular set of rocks in one particular place was described and illustrated in comparison with what was already known of similar floras elsewhere. The types new to science were specially distinguished and compared with what appeared to be their nearest relations among known genera or species. The occurrence of other forms already well known to science was duly noticed, with special reference to the occasion on which they had been previously described and figured. All new and rare specimens were so far as possible illustrated. This work was thus purely systematic.

Among the many authorities of the pioneer period it will only be possible here to indicate a few of those who laid the foundations of this work especially in this country. Some of these wrote perhaps only a single short memoir on a fossil flora of one particular age from a certain district. Several of these contributions are, however, just as much landmarks in the history of the science as the larger works of others, which roamed indiscriminately over the whole range of fossil plants of every geological age and from every region where their occurrence was known to the authors. The earliest memoir, in which fossil plants of any age and from any country were binomially described, was, as we have already mentioned, published in America in 1818, by the Rev. Henry Steinhauer in the *Transactions* of the American Philosophical Society of Philadelphia (N.S., vol. i, No. 18, p. 265, 1818). The plants studied were a few Lycopods and Calamites, chiefly from the Yorkshire and Somerset coalfields, and all were referred to a single genus *Phytolithus*. Excellent figures were given of the

specimens. One of them (Plate LI, Fig. 10) survives as a type, *Sigillaria tessellata* (Steinh.), in addition to *S. notata* (Steinh.), which by some authorities has been regarded as a distinct species. But for the very limited number of fossils described, this memoir would no doubt rank as of much greater importance than it does. It should, however, be noticed that it antedated all the palaeobotanical writings of both Brongniart and Sternberg—in fact, all scientific works on fossil plants. So far as I am aware it is the earliest printed memoir in any country in which binomial terms were applied to fossil plants. The example set by Schlotheim and Steinhauer was swiftly followed by the publication by a Bohemian, Kaspar Maria, Graf von Sternberg (1761–1838), of the *Versuch einer geognostisch-botanischen Darstellung der Flora der Vorwelt*, Hefte I–VIII (1820–38), the first part of which appeared in the same year as Schlotheim's second work. In 1828 there also appeared the first instalment of the great *Histoire des Végétaux fossiles* (1828–38) by Adolphe Théodore Brongniart (1801–76). In an earlier paper published in 1822 Brongniart had already applied generic and specific terms to certain fossil plants (' Sur la classification et la distribution des végétaux fossiles ', *Mém. Mus. Hist. Nat.*, vol. viii), and a complete ' Prodrome ' of his *Histoire* had appeared in 1828: The works of Schlotheim, Sternberg, and Brongniart, which appeared in whole or in part between the years 1804 and 1838, may be regarded as the foundations of systematic Palaeobotany. The work of Schlotheim as regards fossil plants, if limited, was good in quality, but of the three writers Brongniart shows forth as the greatest scientist. His *Histoire*, now a rare and valuable work but still in every-day use, is the most cherished possession of the palaeobotanical researcher, and the one of the older works to which he turns most frequently. One of his plates is shown on a reduced scale in Plate LII, Fig. 11. It is no exaggeration to say that Brongniart first described a larger number of the Coal Measure species which occur in Britain than any other author before or since, and many of his determinations were founded on British specimens. By 1825 appeared Artis's *Antediluvian Phytology*, the first scientific work published in Britain on British fossils by a British subject. Edmund Tyrell Artis (1789–1847) was an archaeologist of some note in Northamptonshire. His incursion into fossil Botany was a brief one, and he only published this one small volume, containing twenty-four


Fig. 11. A. Brongniart, *Histoire des Végétaux fossiles*, Livr. V, 1830, Pl. 68 (reduced). 1, *Neuropteris grangeri*, Brongn., 2, *Neuropteris flexuosa*, Sternb.


Fig. 12. E. T. Artis, *Antediluvian Phytology*, Pl. VII, *Filicites Osmundae* = *Neuropteris Osmundae* (Artis)

plates of Coal Measure plants from Yorkshire. But the work was thoroughly up to date, and the binominal system as applied to fossil plants, then only seven years old, was here fully expressed. The scheme of genera is also much more varied than in any work preceding it. Most of Artis's fossils are types, the specific names of which are still in use, e.g.,

*Pecopteris Miltoni* (Art.).
*Pecopteris* (*Dactylotheca*) *plumosa* (Art.).
*Alethopteris decurrens* (Art.).
*Neuropteris osmundae* (Art.) (Plate LII, Fig. 12).
*Megaphyton frondosum* (Art.).

Artis's book was followed in 1831 by the first instalment of Lindley and Hutton's *Fossil Flora of Great Britain* (1831–7), the three volumes of which contained 230 plates, with explanatory text. Plate LIII, Fig. 13, is a reproduction of a typical plate. This work was the product of a combination of scientist and collector. John Lindley (1799–1865) was undoubtedly one of the greatest botanists of the Early Victorian period, not only in England but in the world. He occupied for many years the Chair of Botany at University College, London, and is particularly famous for his work on the systematic classification of plants and on the Orchid family. William Hutton (1798–1860) was a Newcastle merchant, who was an ardent amateur collector of fossil plants, but there is no evidence that he possessed any scientific knowledge of them. The arrangement between these authors was that Hutton in Newcastle got together as many examples of the flora as he could; then he had drawings made and sent the illustrations, and occasionally the specimens also, from time to time to Lindley, the locality records often getting lost or mixed on the way. Lindley described the plants, and the results were published in parts conjointly. The plants figured in the *Fossil Flora* were of every geological age, from Lower Carboniferous to Tertiary, though a majority were derived from the Coal Measures of Britain. The figures on the whole were exceedingly inaccurate, and have caused endless trouble to later workers. Dr. Kidston in 1891, however, went into the whole matter by a comparison of the plates with the original specimens so far as they could be traced, with a view to deciding what was of value and what was useless among these records. Either the artists employed had no skill in the accurate

representation of such objects, or, as is more probable, neither authors nor artists had any conception of the absolute necessity for strict faithfulness as regards detail in the illustrations. This is a fault common to much of the older literature, though Brongniart's *Histoire* is a conspicuous exception in this respect. It is only when in actual practice we come to compare a specimen with a published figure with a view to deciding whether they are or are not identical, that the need for absolute accuracy in the drawing comes home to us. The descriptions of the plates, chiefly by Lindley, are not brilliant ; and, for the reasons already indicated, the records of ' locality ' are sometimes unreliable. But in one respect these volumes were good—the nomenclature applied to these fossils. The majority of the specimens figured in Lindley and Hutton are preserved in the Hancock Museum, Newcastle-on-Tyne.

I have entered somewhat fully into this work because it unfortunately remains to this day the one illustrated British book containing a general account of our Coal Measure plants. It is, however, far superior to its only British contemporary, Edward Mammatt's *A Collection of Geological Facts and Practical Observations intended to elucidate the formation of the Ashby Coal-field*, which appeared in 1834. This is a large quarto, containing no fewer than 102 plates devoted to the fossil flora of the particular coalfield. The text relating to the plates occupies only five pages, and is restricted to the names of the fossils and previously published figures of the same. Both the fossils themselves, the quality of the illustrations, and the absence of descriptions, combine to render this handsome work mere waste paper so far as a knowledge of Coal Measure plants is in question. With the completed publication of Lindley and Hutton's *Fossil Flora* in 1838, the pioneer period of Scientific Palaeobotany in this country was fairly begun.

### (ii) *The Early Scientific Study of Petrifactions*

We now have to turn to other matters affecting certain fossil plants preserved in a particular manner. It had long been known that in various regions petrified woods occasionally occurred, in which the anatomical structure was more or less preserved by replacement by a mineral salt such as calcite or silica. The invention of the microscope led naturally to the idea of investigating

Fig. 13. J. Lindley and W. Hutton, *Fossil Flora of Great Britain*, Vol. III, 1837, Pl 222, Figs. 1 and 3, *Trigonocarpum olivaeforme*; Figs. 2 and 4, *T. nöggerathi.*

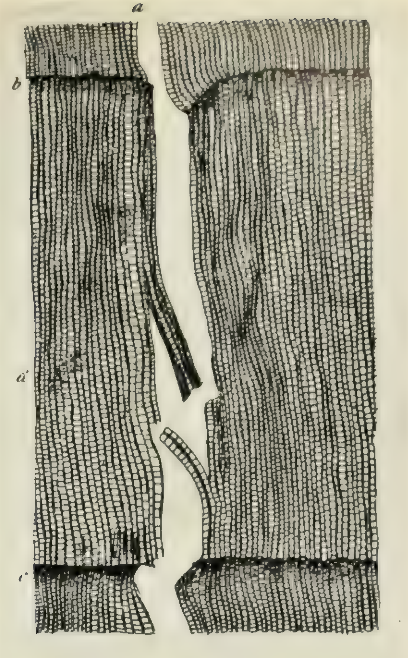


Fig. 14. W. Nicol, *On Fossil Woods from Newcastle, New South Wales.* Edin. New Phil. Journ., Vol. 14, 1833, Pl. III, 1, section of 'petrified conifera'.

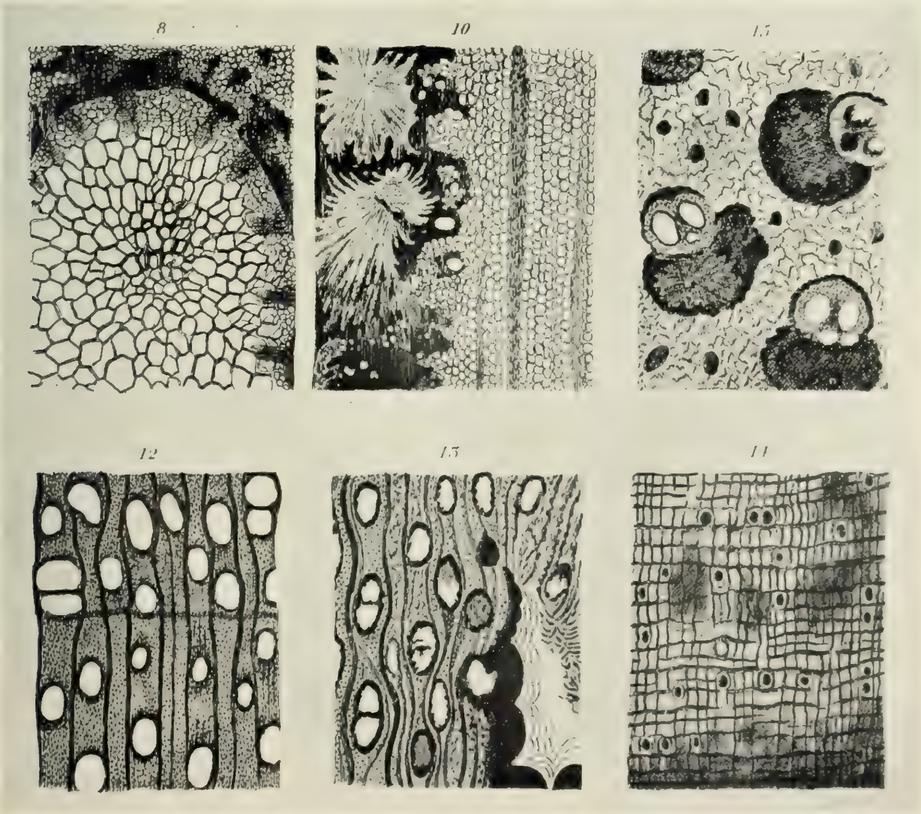


Fig. 15. H. T. M. Witham, *Observations on Fossil Vegetables*, 1831, sections of petrified tissues.

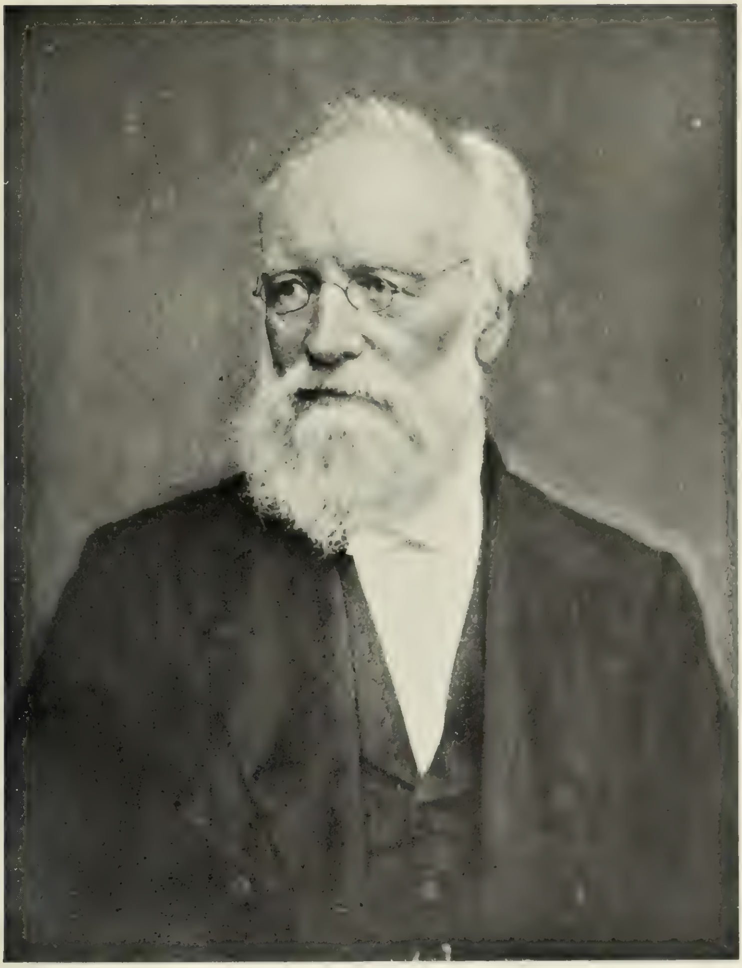
the anatomy of these fossils. Who it was who first overcame the initial difficulties of preparing sections of these woods sufficiently transparent to transmitted light and thus rendered them capable of being examined under the microscope, is a somewhat disputed point. The credit has generally been given, and I believe rightly, to William Nicol (? 1768–1851) of Edinburgh, who is remembered as the inventor of the Nicol prism. Nicol himself gave the credit to his lapidary, Sanderson, who in his turn had no doubt been inspired by Nicol himself. At any rate, Nicol did actually form a collection of thin slices of such woods, most of which are now in the British Museum, though a few are at Cambridge and in Stockholm. He published four papers on these between 1831 and 1835 in the *Edinburgh New Philosophical Journal* (vols. x, xiv, xvi, and xviii), but none of the specimens which he described were from the British Coal Measures. Plate LIII, Fig. 14, shows one of his drawings. After the completion of the publication of the third volume of Lindley and Hutton's *Fossil Flora* in 1837, the record of original work in Britain on fossil plants is almost a complete blank until about 1870, when the modern period commences. During this interval of more than thirty years, several famous botanists made brief excursions into the domain of fossil plants, notably Robert Brown (1773–1858) in 1851, and Sir Joseph Hooker (1817–1911) in 1848. John Morris (1810–86), a geologist in London, appears, however, to have been regarded as the chief authority on Carboniferous plants during this period, but he did little original work. His *Catalogue of British Fossils*, which went through two editions in 1843 and 1854, is, however, remarkable not only for the completeness with which all British fossil plants previously recorded were indexed, but as being the first and so far the last work of its kind published in this country. There is, however, one name of importance which in Britain served as a link between the days of Lindley and Hutton's flora and the modern school—Edward William Binney (1812–1881). Binney, a Manchester coal and paraffin merchant, was an amateur geologist and palaeobotanist who produced over a period of many years a large number of very modest contributions on all sorts of Palaeozoic fossil plants. Of these, those which relate to Carboniferous petrifactions are the most important, and form a connecting link between the works of Witham and Williamson in the history of the anatomical study of British fossil plants. Binney's memoirs on these matters, which

especially related to Carboniferous Lycopods and Equisetales, and the seed *Trigonocarpus*, were published between 1855 and 1872.

The earliest results from Coal Measure materials to be published were from the pen of another writer, who repeatedly acknowledged his indebtedness to Nicol not only for material but for the method of obtaining the thin sections. This was Henry Thomas Maire Witham of Lartington in Yorkshire (1779–1844). In 1831 Witham published the first of two works, of similar contents, on the structure of fossil woods from various parts of the world. This was called *Observations on Fossil Vegetables, accompanied by representations of their internal structure as seen through the microscope*, and it was dedicated to William Nicol. The latter part of the title is particularly significant in the history of the study of fossil plants. Plate LIII, Fig. 15, shows some of Witham's anatomical drawings. In the same year Witham also published a memoir on the internal structure of a very important Coal Measure plant, *Lepidodendron Harcourtii*, With. These are the earliest memoirs illustrating microscopic sections published in any country. The second and augmented edition of Witham's book appeared in 1833, only two years later than the first, under the title *The Internal Structure of Fossil Vegetables found in the Carboniferous and Oolitic Deposits of Great Britain.* In the first edition the plants were not named binomially. They consisted of sections of the Lower Carboniferous genus *Pitys* from the neighbourhood of the Tweed and Edinburgh, Coal Measure Dadoxylons from the Newcastle coalfield, fossil woods from the Lias of Whitby, ? Permo-Carboniferous wood from Australia, and Tertiary wood from the Island of Eig or Egg (*Pinites Eggensis*, L. & H.), with Dicotyledons and Palms from Antigua, also of Tertiary age. In the second edition the chief additions were *Lepidodendron Harcourtii* and *Anabathra pulcherrima*, the latter a Lower Carboniferous type. In this edition some of the plants were named binomially.

There was little in these early works of Witham and Nicol which related to the flora of the Coal Measures, though Witham did, in fact, describe a Dadoxylon of that age from the Newcastle coalfield. These researches, however, are very important as being the forerunners of the great anatomical school of Williamson in later years, and of the modern period. With this later period the work of Witham and Nicol is linked by a few papers on the petrifactions of the Coal Measures described by Lindley and Hutton

PLATE LIV



W. C. Williamson

Fig. 16. William Crawford Williamson 1816-1895

in their *Fossil Flora* (1831–7); memoirs on this subject were published by Sir Joseph Hooker in 1848, Robert Brown in 1851, Hooker and Binney in 1855, and William Carruthers in 1867–70.

The earliest work on Carboniferous petrifactions on the Continent was Brongniart's classic on *Sigillaria elegans*, published in 1839, so in this path of progress the English school were distinctly first.

This period ends with the scientific discovery of 'coal balls' in 1868, first foreshadowed by Binney between 1855 and 1862, on which he himself, and in a still greater degree Williamson (Plate LIV) working simultaneously, but independently, established a few years later the anatomical side of palaeobotanical study on a sure footing (Plate LV, Fig. 17, is an example of Williamson's drawing).

### (iii) *The Intermediate Period in the Study of Incrustations*

With regard to 'impressions', the modern period in Britain starts about the year 1881, when Dr. Kidston published his first scientific paper, and on the Continent some five years earlier.

We have now to consider what was being done in this country at Coal Measure impressions between 1837 (when Lindley and Hutton's third volume was completed) and 1882. This period of nearly half a century undoubtedly represents a great gap in the history of progress so far as the Coal Measure plants of this country are concerned. John Morris (1810–86), Professor of Geology at University College, London, was the chief authority on such fossils in the fifties, but he did little that was of an original nature. For the rest we have very brief but important contributions by Binney and by Carruthers, and that is all. Comparatively speaking, this period of forty-five years is a blank in so far as Coal Measure plants are concerned, though some progress was made with other floras. The object of the workers of the earlier or pioneer stage of the scientific period was, as a rule, only to record and figure. An illustration was given of a particular fossil which was named binomially and the record of its place of origin was also added. Little more was attempted. The outlook was wholly or chiefly taxonomic. If comparisons were embarked upon, it was usually merely in order to discuss the fossil in connexion with some living plant, often with a view to showing its identity with, or close relationship to, the recent type. Perhaps the outcome of these discussions appears to-day to be more negligible

than it really was, owing to the constant error of all the older workers, not one of whom appears to have ever even dreamed of how remote the Carboniferous flora is from the British flora of to-day, both in time and in affinity. This mistaken outlook curiously enough lingers most persistently even among some modern workers; this is all the more remarkable, seeing that it is the achievements of this very school itself which have placed the facts beyond doubt. Speaking generally, it is still not sufficiently recognized how very remote this flora is in affinity from the vegetation of to-day of any part of the globe. It is true that we now know better than to attempt to include types of Carboniferous Pteridosperms in recent genera of Ferns, but we are still too anxious to assign fossils of that age to living families or groups. For the reasons just indicated, it is thus the descriptive rather than the systematic results of the pioneer period which are of value. This type of work naturally is not confined to the period in question. A very large proportion of modern publications on fossil plants are purely systematic and descriptive. And such will always continue to be the case. These are the necessary foundations on which everything palaeobotanical is built. Until a fossil is named, figured, and described it does not exist scientifically.

### (iv) *The Modern Period*

The peculiarity of the researches of the modern period, which dates from about 1870, is that while purely descriptive records have been continued unabated, yet a large number of investigations of a special nature have also been undertaken. These may perhaps be best described as intensive cultivations of fossils, in many cases already known to science. A set or group of fossils has been reinvestigated more minutely and on broader lines than would be suggested by a purely taxonomic outlook. The results of these reconsiderations have often been most remarkable, e.g. the establishment of the group Pteridospermeae in 1903. These investigations are to a very large extent purely botanical in outlook and have as much a bearing on recent Botany as on all branches of Palaeobotany. At the same time the geological applications of other studies relating to the occurrence and distribution of Coal Measure plants have proved very important, not only on the pure, but even on the applied side of Geology, and some of these again affect botanical notions of evolution and kindred questions. It is

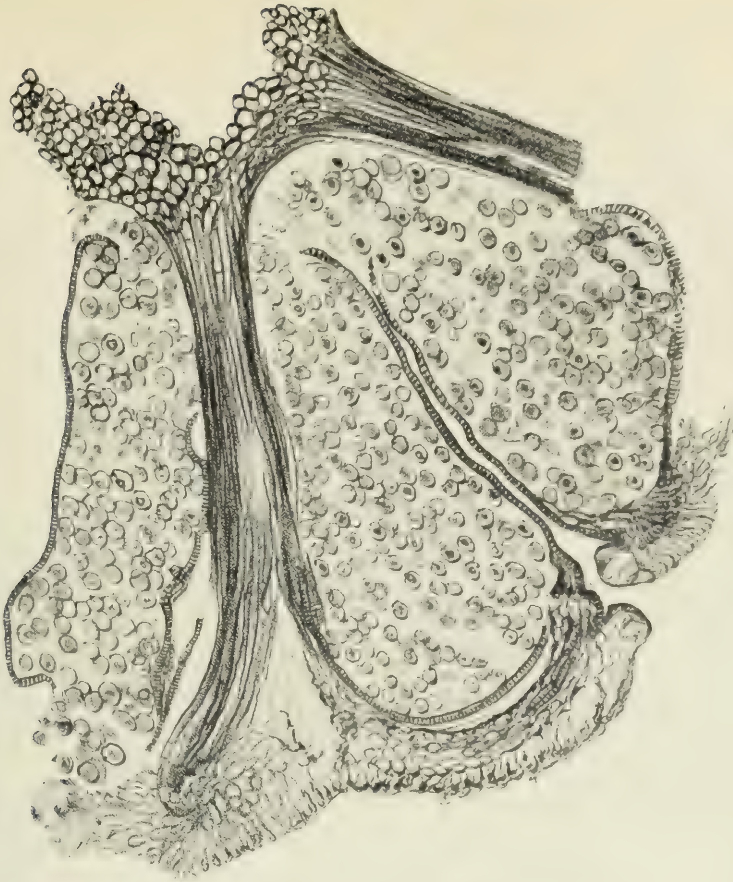


Fig 17. *Calamostachys Binneyana*

From a drawing by W. C. Williamson of the transverse section of the cone

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Hornets drawn by Thomas Mouffet before 1589

quite impossible to enter here into these advances in detail; to do so would be to set forth the present position of the subject in all its aspects—a long and laborious undertaking. Suffice it to say that these results, considering that barely half a century has elapsed since the beginning of the period, are certainly very remarkable in many directions and offer great inducements to future workers. If at first they hardly attracted the attention which they deserved, they have, since about the year 1895, received full recognition by the kindred sciences, especially Botany. This modern renaissance arose independently both in Britain and in France at almost the same period. Both schools have been very closely in touch with one another, and each has helped and learnt from the other: while naturally much good work has been done in many other countries, there has been no such body of continuous and linked research on fossil plants as has existed during the last fifty years in France and England. If anything, the French began a few years earlier (Renault 1868, Grand'Eury 1877, Zeiller 1878), but the dates of Williamson's and Kidston's first special researches are so nearly simultaneous, and they arose so independently, that the difference is negligible. Most of the initiators of the great French Modern School have now passed from us, including the two outstanding names of Charles René Zeiller (1847–1915), that Prince of Palaeobotanists, and Bernard Renault (1836–1904). In this country we have lost William Crawford Williamson (1816–95), the Father of Palaeobotanical Anatomy (Plate LIV, Fig. 16), but most of the other workers are happily still with us, among whom the names of Dr. D. H. Scott and Dr. R. Kidston are household words to all interested in Coal Measure plants.

# ARCHIMEDES' PRINCIPLE OF THE BALANCE AND SOME CRITICISMS UPON IT

By J. M. Child

No modern student of the history of the science of mechanics can start without confessing indebtedness to the brilliant work of Ernst Mach, and especially to his *Science of Mechanics*. There are certain points of criticism in it, however, with which one cannot agree; and some, indeed, which the average mathematician fails to grasp, through lack of analytical power. The possession of this, however, seems prone to obsess its owner, and to overpower his imaginative qualities. Now, it is just these imaginative qualities that must be drawn on to a large extent, when discussing the work of the ancient Greeks. Thus, it is Mach's criticism of the work of Archimedes that has given rise to this Essay.

In his *Science of Mechanics*, p. 14,[1] Mach writes:

'From the mere assumption of the equilibrium of equal weights at equal distances is derived the inverse proportionality of weight and lever-arm! How is that possible? If we were unable philosophically and *a priori* to excogitate the simple fact of the dependence of equilibrium on weight and distance, but were obliged to go for *that* result to experience, in how much less a degree shall we be able, by speculative methods, to discover the *form* of this dependence, the proportionality!

'As a matter of fact, the assumption that the equilibrium-disturbing effect of a weight $P$ at the distance $L$ from the axis of rotation is measured by the product $P.L$ (the so-called statical moment), is more or less covertly or tacitly introduced by Archimedes and all his successors. For when Archimedes substitutes for a large weight a series of symmetrically arranged pairs of small weights, which weights *extend beyond the point of support*, he exploys in this very act the doctrine of the centre of gravity in its more general form, which is itself nothing else than the doctrine of the lever in its more general form.

[1] By this title is meant the English edition published by the Open Court Pub. Co., 1907; trans. McCormack. The supplementary volume, edited by P. E. B. Jourdain, 1915, will be referred to as Mach's *Supplement*.

'Without the assumption above mentioned of the import of the product *P.L*, no one can prove that a bar, placed *in any way* on the fulcrum *S*, is supported, with the help of a string attached to its centre of gravity and carried over a pulley, by a weight equal to its own weight (Fig. 1). But this is contained in the deductions of Archimedes, Stevinus, Galileo, and also in that of Lagrange.'

That the view taken by Mach is not beyond criticism is shown by the fact that, in two notes on pp. 513, 514 of the *Science of Mechanics*, Mach himself mentions an essay by O. Hölder *Denken und Anschauung in der Geometrie*, in which the author upholds the correctness of the Archimedean deductions against the criticism of Mach. Further, in the *Supplement*, Mach calls attention to a 'beautiful paper by G. Vailati', *La dimostrazione del principio della leva data da Archimede*, which appeared in the *Bollettino di bibliografia e storia delle scienze matematiche* for May and June 1914. In this paper, Vailati takes, as Mach says,

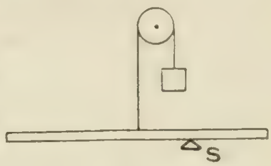

FIG. 1.

'the side of Hölder against my criticism of Archimedes' deduction of the law of the lever, but partly too Hölder is criticized. I believe that every one may read Vailati's exposition with profit and, by comparison with what I have said on pp. 17–20 of the third edition of my *Mechanics*, will be in a position to form a judgment upon the points at issue.'

Thus the aim of Mach, as he says later in the note, has been to strive to disturb the impression that the general law of the lever could be deduced from the equilibrium of equal weights on equal arms; and thereafter to show where the experience that already contains the general law of the lever is introduced. Hölder argues against the correctness of Mach's criticism, while Vailati shifts the ground of the debate, by suggesting that Archimedes 'derives the law of the lever on the basis of general experiences about the centre of gravity'. Mach concludes his note by remarking that 'if the reader has derived some usefulness out of this discussion, I am not very particular about maintaining every word I have used'.

The third paragraph of the quotation from the *Science of Mechanics*, as given above, can be dismissed. In the fourth German edition, it is omitted in this connexion; and, on p. 514 of the *Science of Mechanics*, there is a direction that, instead of this

paragraph, there should be inserted on p. 20, at the end of the first paragraph, the passage that is given below ; it is to be noted that there is no direction given, as there should be, for the deletion of the paragraph from p. 14. The diagram is the same (Fig. 1), and the new context reads :

'A knife-edge may be introduced at any point under a prism suspended from its centre without disturbing the equilibrium, and several such arrangements may be rigidly combined together so as to form apparently new cases of equilibrium. The conversion and disintegration of the case of equilibrium into several other cases (Galileo) is possible only by taking into account the value of $PL$.'

This cannot possibly be contained in the deductions of Archimedes, for according to Archimedes it is wrong, since to a balanced load there cannot be added an unbalanced load without disturbing the equilibrium. Using modern phraseology, if the knife-edge S were placed *in any manner, at any point of the bar except at its middle point, so as to actually touch the bar even in the slightest degree*, the equilibrium would be disturbed ; in fact such an arrangement might be used as an indicator of the first moment of contact with extreme delicacy. The revised version of the paragraph certainly does not state that the equipoise is equal to the weight of the bar, at least not directly ; but this is the only conclusion we can come to, if we consider that the bar is supposed to be suspended from its centre before the knife-edge is introduced, and secondly that the two figures are identical.

The second half of the second paragraph ought also to have been noted for deletion. Its place is taken by the following passage (p. 513) :

'It is very obvious that if the arrangement is absolutely symmetrical in every respect, equilibrium obtains on the assumption of any form of dependence whatever on the disturbing factor $L$, or, generally, on the assumption $P.f(L)$ ; and that consequently the particular form of dependence *$PL$ cannot possibly* [1] be inferred from the equilibrium. The fallacy of the deduction must accordingly be sought in the transformation to which the arrangement is subjected. Archimedes makes the action of two equal weights to be the same under all circumstances as that of the combined weights acting at the middle point of their line of junction. But,

[1] I have here correctly given the words to be in italics ; McCormack has wrongly italicized the word 'particular'. The original is : 'demnach kann aus diesem Gleichgewicht unmöglich die bestimmte Form $PL$ abgeleitet werden.'

seeing that he both knows and assumes that the distance from the fulcrum is determinative, this procedure is by the premise unpermissible, if the two weights are situated at unequal distances from the fulcrum. If a weight situated at a distance from the fulcrum is divided into two equal parts, and these parts are moved in contrary directions symmetrically to their original point of support ; one of the equal weights will be carried as *much towards* [1] the fulcrum as the other weight is carried *from* [1] it. If it is assumed that the action remains *constant* [1] during such procedure, then the particular form of dependence of the moment on $L$ is implicitly determined by what has been done, inasmuch as the result is only possible provided the form be $PL$, or be *proportional* to $L$. But in such an event all further deduction is superfluous. The entire deduction contains the proposition to be demonstrated, by assumption if not explicitly.'

Leaving the criticism of these passages for a while, and coming to the historical side of the question, we find that Mach gives prominence only to the work of Huygens, Lagrange, Leonardo da Vinci and Galileo, with a mere passing reference to Guido Ubaldi del Monte. There is no mention of Guldinus's work, *Centrobaryca*, or *De Centro Gravitatis*, in which that author starts with the Postulates of Archimedes (of which he quotes nine), three axioms of Guido Ubaldi, the hypothesis that there is but one centre of gravity, followed by eight Propositions, with corollaries, taken from the first book of Archimedes ' on Equiponderants '. From this work it would seem that Vailati's view was not the view taken by Guldinus. For we cannot assume that Guldinus would found the whole of his work on Archimedes' Principle of the Balance, if he had thought that this principle had been obtained from the consideration of general experiences about the centre of gravity. Guldinus quotes freely from a commentary on the works of Archimedes written by Guido Ubaldi ; this commentary is not mentioned by Mach.

Then, again, there is no mention made of the work of Cavalieri, given in the fifth section of his *Exercitationes Sex*, wherein is to be found the definition of the term ' moment ', and that too in phraseology which seems to suggest that it was first used by Cavalieri as a definite nomenclature ; and this, again, suggests that he was the first man to *definitely* see its importance as

[1] The equivalents in the German were set for italics ; I have also given a more correct rendering of ' nähert ', the words ' near to ' having an ambiguous meaning.

a *principle*, apart from haphazard use of it in practice. He notes the fact that a body may have different moments according to its position. Coupled with this work is the work of Pascal on the cycloid; the first section of 'The Letters of Amos Dettonville', which contained the 'Method of the Centre of Gravity', may be assigned with almost certainty to a study of the above-mentioned exercitation of Cavalieri; the whole of Pascal's method depends on the principle of the 'balance', and yet Pascal is not mentioned in this connexion by Mach. These two men, Cavalieri and Pascal, gave Leibniz the idea for his centrobaryc analysis, and are, if for that reason alone, of extreme importance. For Leibniz inverts the usual process, and obtains a fundamental theorem in the calculus from the theorem that the moment of an area is equal to the sum of the moments of its parts.

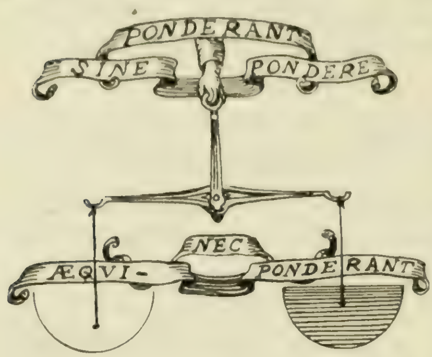

Fig. 2.

Now the work of Cavalieri and Pascal was, and is, considered as an extension of the work of Archimedes; with Cavalieri and Pascal, it is the equilibrium-effect and not the turning-effect, or any disturbing-effect, that is always considered; even though the former defines and uses the 'moment', after he has derived the idea from Archimedes, while Pascal expressly uses the word 'balance'. In fact, it is not the law of the *lever*, but of the *balance*, or even more correctly the law of *balanced things*, that is primarily considered by Archimedes, and applied geometrically for geometrical purposes: thus the Latin title of 'Equiponderants' is a most suitable rendering. What Guldinus thought of the principle is evident from the vignette on the title-page of the first volume of his *De Centro Gravitatis*, a freehand sketch of which is here given (Fig. 2).

Of course it is not suggested that Archimedes was unaware at the start of the proportionality of the arms of the balance. Gow, in his *History of Greek Mathematics*, states that Aristotle remarks that cheating tradesmen would shift the centre of their balances towards the scale in which the weight lay (*Mech. Prob.* 1, fin.); and remarks that weights in equilibrium on a lever are inversely proportional to the arms inversely. Even if, as con-

sidered by Wohlwill (see *Supplement*, p. 6), the 'Mechanical problems' are not by Aristotle, there is every possibility, if not probability, that the law was known to Archimedes. My contention is that Archimedes, 'in his Grecian mania for demonstration, strives to get round this', as Mach himself says, on p. 28 of the *Science of Mechanics*; and that he succeeds without *the introduction of the idea of moments*; which is in opposition to Mach, who finishes his sentence with the words, 'his deduction is defective'.

I propose, therefore, to endeavour to show how the deductions of Archimedes might have been obtained without even a knowledge of the principle of moments. Since these deductions lead to the inverse proportionality of the arms of the balance, my first step must be to show that *the principle itself is actually contained in the postulates*, and therefore that there is no need for it to have been introduced by Archimedes at any intermediate step of his reasoning.

Of the nine postulates, the only ones that concern us at present are:

1. Equal weights at equal distances balance.
2. If weights balance at any distances, and to one of them a weight is added, then they will not balance; but there will be a downward motion of the weight to which something has been added.
3. Similarly, if something is taken away from one of the weights, they will not balance; but there will be a downward motion of the weight from which nothing has been taken away.
8. If magnitudes balance one another at any distances, things that are equal to these magnitudes will also balance at the same distances.

Now, as Mach states (p. 513, see quotation given above), these postulates contain the principle that the distance is determinative; in other words, that for a given weight set at a given distance, there is (i) *but one weight at a given distance*, and (ii) *but one distance for a given weight*, with which a balance can be effected.

For, it is not too much to assume the 'common notions', stated by Guido Ubaldi, in addition to these postulates. The first two of these axioms are respectively:

1. If from equiponderants, equiponderants are taken away, the remainders are equiponderant.

2. If to equiponderants, equiponderants are added, the wholes are equiponderant.

These together form a statement of what is called 'the principle of the superposition of loads'; and this principle is one of the, if not *the*, most important fundamental principles of mechanics; apparently Mach does not consider it, perhaps on account of its obvious nature. The third axiom is:

3. Weights that balance the same thing are equal.

This, in other words, is the statement that, if there are two weights, for any given distance, that will balance a given weight at a given distance, then these two weights must be equal; that is, there is but the one weight for the given distance. Further, if there are supposed to be two distances for one given weight, that will effect a balance with a given weight at a given distance, then two equal weights will balance one another at these unequal distances; which is obviously contrary to the postulates.

Thus, in modern phraseology, there is established a one-to-one correspondence between $P$ (the weight) and $L$ (the distance at which it is set), in order that a balance should be established with a given weight at a given distance. If we now make the assumption, made by Mach without any statement as to the grounds of the assumption, that $P$ is some function of $L$, we can make, to start with, a more general assumption as to the form of the function. The assumption of a function is justified on the hypothesis of the simplicity and regularity of nature; Mach says that 'equilibrium obtains on the assumption of *any* form of dependence whatever on the disturbing factor $L$, or, generally, on the assumption $P.f(L)$'; but this is hardly justifiable. If we consider ourselves justified in assuming functionality, we must, at any rate to start with, make a more general assumption. The equilibrium-effect (note that it is not necessary to suppose a turning-effect, or any disturbing-effect of any sort, or to consider such in any way), must be the general function $f(P, L)$. It is not until we consider the one-to-one correspondence that we can say that this function must be of the form

$$a.P.L + b.P + c.L + d.$$

But, having obtained this form of the function, it is evident that it must be zero when (1) $P=O$, (2) $L=O$, (3) $P=O$ and $L=O$; hence $b$, $c$, $d$, are all equal to zero, and thus the effect is *proportional*

to *P.L.* In other words, *the inverse proportionality of the arms is contained in the postulates*; and it only remains for Archimedes to deduce it by accurate reasoning with the means at his disposal; and this he can do *without introducing the idea of the statical moment at any step in the course of his reasoning.*

The next step we have to make is to give Archimedes' reasoning, and Mach's version of it; and to criticize the latter. To do this, it is necessary to give a summary of the first six propositions of Archimedes.

Prop. I states that 'weights that balance at equal distances are equal'; Prop. II states that 'unequal weights at equal distances do not balance, but give a preponderance on the side of the greater weight'; Prop. III states that, 'for unequal weights at unequal distances, the greater weight is at the less distance'. These are merely more or less obvious converses to the postulates, or rather corollaries to them. In Prop. IV there is a transition from weights to magnitudes (from βάρεα to μεγέθεα in the Greek), and we have the introduction of the term that is usually translated 'centre of gravity' (κέντρον τοῦ βάρεος). Archimedes proves, by a *reductio ad absurdum* depending on the second postulate, that 'the centre of gravity of a combination of two equal magnitudes is at the middle point of the line joining the centres of gravity of the two magnitudes'. In the proof Archimedes states that the fact that it lies in the straight line has been already proved, and the remark is merely repeated by Eutocius in his commentary; we are left in doubt as to whether this proof has been given elsewhere by Archimedes or by others. The term κέντρον βάρεος had been invented before the time of Archimedes, according to Gow, who seems to have considered the fact that Archimedes uses it without defining it; just as I use the fact, that Cavalieri carefully defines the term *momentum*, to reason that Cavalieri was the first to consider the term as signifying something equivalent to what we now call a moment, or at least to use it in this definite sense. That this was not the general sense of the word *momentum* is clearly shown by the preliminary note made by Eutocius.

'Both Aristotle, and Ptolemy after him, teach us that the tendency downwards (ῥοπή) is a certain sort of gravity and levity common to all things. Further, in the works of Plato, Timaeus says that all this downward tendency comes from gravitation; indeed, he

thinks that levity is but a lack of it. . . . Now, in this book, Archimedes considers that the centre of this downward tendency [this ῥοπή, which Torelli translates by 'momentum *quod dicitur*'] is a point, such that if the plane, of which it is the centre, is suspended by this point, it will still remain parallel to the horizon; and for two, or more, planes it is the point such that, if the balance is hung from this point, it will remain parallel to the horizon.'

Thus, with Archimedes, there is no idea of a centre of parallel forces; the centre is merely the *balance-point*. The modern equivalent is *centroid*. The whole nomenclature is merely for *a pictorial purpose*; it is just as if Archimedes had said: You know that equal weights will balance at equal distances, and so on; well, the same idea (*and it will be useful to employ the same language*) can be adapted to geometrical reasoning about planes and things, *which have no weight*. We will consider that they have a 'weight-centre', at which we can consider that the whole plane is condensed; and that a balance can be established between two planes, and suchlike, just in the same way as with weights hung on a balance. Then, Archimedes, in Prop. IV, starts his work on planes; this start being marked by the transition in the original from 'weights' to 'magnitudes'. I think there is corroboration of this in the fact that, in later work, Archimedes uses an axis of balance, and not merely a point of suspension, without making any reference to the change. With this view, also, it would have been impossible for him to have given a geometrical proof that, when condensing his planes at their 'weight-centre', the magnitude at the centre is the sum of the magnitudes of the planes that are condensed; which is the equivalent of the fact that, with weights on a balance, the force on the fulcrum is the sum of the suspended weights. This assumption is made by Mach without any remark; and it is a greater assumption than any that he accuses Archimedes of making, and *itself includes the whole 'law of the lever'*. For, if two equal weights are supported by a double weight at the centre, we have, by taking moments round either end of the balance, the fact that a given weight will be in equilibrium with one twice as large, when the arms are in the ratio of 1 to 2. The proof for balanced planes depends on the first postulate, the superposition of loads, and the postulate that two loads that balance a given load are equivalent to one another. Thus suppose that $M$ and $N$ (Fig. 3) are two equal

magnitudes having $A$ and $B$ as their centres; and let $C$ be any other point not on the line $AB$. Join $AC$, $BC$; let $D$, $E$ be their middle points; join $DE$, and draw any line $CFG$ to cut $DE$ in $F$, and $AB$ in $G$. Then it is clear that $CF$ is equal to $FG$. Now the magnitude $M$ at $A$ can be balanced about $DE$, i.e. about the middle point of $AC$, which is on $DE$, by a magnitude equal to $M$ at $C$; and similarly, $N$ at $B$ can be balanced about $DE$ by a magnitude equal to $N$ at $C$. Therefore $M$ and $N$ together can be balanced by a magnitude equal to the sum of $M$ and $N$, situated at $C$. But the latter can be balanced by a magnitude equal to the sum of $M$ and $N$, situated at $G$, since $GF = FC$. Therefore, $M$ at $A$ and $N$ at $B$ are equivalent to the sum of $M$ and $N$ at $G$, some point on the straight line $AB$; *if, and only if, it is known that the 'weight-centre' of* M *at* A *and* N *at* B *is on the line* AB.

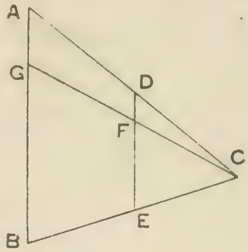

FIG. 3.

Thus the fundamental postulate is that the centre is on the straight line joining the respective centres of the two magnitudes; Archimedes says this has been proved, and Eutocius merely repeats the assertion, without giving any information as to how, when, where, and by whom the demonstration has been given; this is the reason why I have called it a postulate. It is to be noticed that the above proof only shows that the sum of the magnitudes can be taken at a 'weight-centre', somewhere on the line $AB$; the position of the point on that line has to be determined by other means, since, in the demonstration given, we can only consider an axis of balance parallel to $AB$.

Once this Prop. IV is demonstrated, I therefore contend that the language of Archimedes is merely figurative and pictorial. This view is corroborated by the concluding sentence of the proof of Prop. VI; the Greek is: *τοῦ μὲν ἄρα* A *κειμένου κατὰ τὸ* E, *τοῦ δὲ* B *κατὰ τὸ* Δ, *ἰσορροπησοῦντι κατὰ τὸ* Γ. In this it is to be noted that the preposition *κατὰ* is used in all three cases; and, although this preposition is capable of a variety of meanings, it is highly probable that it would retain the same meaning in the same sentence. Again, although Torelli translates the prepositions by ... *ad* ... *ad* ... *in*, making a change in the third case, it is to be noted that he uses *in* (with the accusative = upon) and not *circa* (= about). I shall, therefore, in what follows, speak of

'a balance at the point $P$, or at the line $AB$'; the latter phrase being short for 'a balance at any point on $AB$'. I shall use the term 'centroid' or 'centre' also, instead of 'centre of gravity', to emphasize the point that it is merely a balance-point, or weight-centre at which the whole plane can be assumed to be condensed, *figuratively* speaking; in this way, I believe I shall give the true interpretation of Archimedes' work.

Prop. V extends Prop. IV to the cases of any number of magnitudes; in it, according to the figures given by Torelli, it is not necessary that the magnitudes should all be equidistant, so long as they are symmetrical. Nor does the demonstration demand this; all that is required is that, for any *pair* at equal

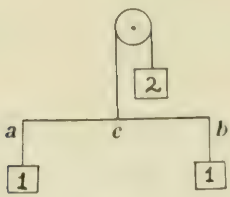


FIG. 4.

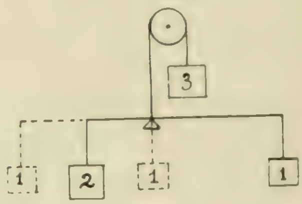


FIG. 5.

distances on each side of the middle, the 'gravities' should be equal. We are here forcibly reminded of the definition of the 'centre of gravity' given by Boscovich in his *Philosophiae Naturalis Theoria,* a definition that is purely geometrical in principle. Boscovich considers all bodies to be made up of points, having equal gravities; and the gravity of a body is therefore proportional to the number of points in it. With this idea, he defines the position of the centre of gravity (retaining, as he remarks, the name, but not using the idea of gravity at all), and proves the existence of the centre for all bodies. The gist of what Boscovich says is as follows. A plane can be found parallel to any given plane, such that the sum of the distances from it of all the points to the right of it is equal to the sum of the distances of all the points to the left of it. Three such planes, mutually at right angles, determine a point; this point is the centre of gravity. Any plane through the centre of gravity has the equal-distances property. If several points are situated at the same place, as in a particle, then the corresponding distance must be taken as many times as there are points in the body; which comes to the same thing as multiplying each such distance by

the mass of the particle. The idea of Boscovich I take to be an almost exact equivalent of that of Archimedes.

Before giving Archimedes' proof of the famous Prop. VI, I will quote, from pp. 10, 11 of the *Science of Mechanics*, Mach's attempt to 'reproduce in general outlines the train of thought by which Archimedes endeavours to reduce the general proposition of the lever to the particular and apparently self-evident case'. The two equal weights 1 suspended at *a* and *b* are, if the bar *ab* be free to rotate about its middle point *c*, in equilibrium. If the whole be suspended by a cord at *c*, the cord, leaving out of account the weight of the bar, will have to support the weight 2. The equal weights at the extremities of the bar supply accordingly the place of the double weight at the centre (Fig. 4).

'On a lever (Fig. 5), the arms of which are in the proportion of 1 to 2, weights are suspended in the proportion of 2 to 1. The

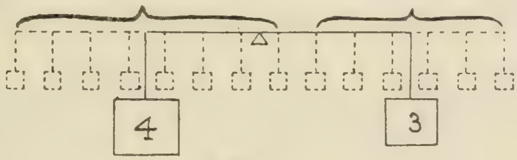


Fig. 6.

weight 2 we imagine replaced by two weights 1, attached on either side at a distance 1 from the point of suspension. Now again we have complete symmetry about the point of suspension, and consequently equilibrium.

'On the lever arms 3 and 4 (Fig. 6) are suspended the weights 4 and 3. The lever arm 3 is prolonged the distance 4, the arm 4 is prolonged the distance 3, and the weights 3 and 4 are replaced respectively by 4 and 3 pairs of symmetrically attached weights $\frac{1}{2}$, in the manner indicated in the figure. Now again we have perfect symmetry. The preceding reasoning, which we have here developed with specific figures, is easily generalized.'

It is upon this 'reproduction of Archimedes' train of thought' that Mach founds his objections; and I contend that all the difficulties are of Mach's own making. First of all Archimedes does not use a pulley, nor apparently does he view the balance in elevation, but in plan. He does not load his 'lever' with weights suspended from the bar; but his bits of planes are attached to the line of centres, anyhow as far as the wording of the demonstration goes, though the figures show them to be laid balanced across the line. He does not produce the bar *after* it has been

once loaded, nor does he *replace* a weight 2 by two weights, each equal to 1. In fact Mach misrepresents Archimedes in every possible way, as will be seen by comparison with the proof of Prop. VI, as given by Archimedes, reproduced below.

### Proposition VI

Commensurable magnitudes will balance at distances inversely proportional to their gravities.

Let $A$ and $B$ be commensurable magnitudes, and let their centres be $A$ and $B$; also let $ED$[1] be any straight line; and suppose that the magnitude $A$ is to the magnitude $B$ as the length $DG$ is to the length $GE$. It is required to show that $G$ is the weight-centre of the magnitude which is made up of $A$ and $B$ taken together (Fig. 7).

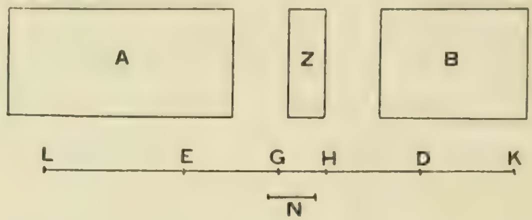


Fig. 7.

For, since $A:B=DG:GE$, and also $A$ and $B$ are commensurable; hence, $DG$ and $GE$ are commensurable, straight line to straight line; therefore they have a common measure; let this be the straight line $N$. Take $DH$, $DK$, each equal to $EG$, and $EL$ equal to $DG$. Then, because $DH=EG$, therefore $DG=EH$; hence also $LE=EH$. Now, $LH$ is double of $DG$, and $HK$ of $GE$; therefore $N$ will measure both $LH$ and $HK$, since it measures their halves. Also, since $A:B=DG:GE$, and $DG:GE=LH:HK$, each being double of each; therefore also $A:B=LH:HK$.

Now, whatever multiple $LH$ may be of $N$, suppose that $A$ is the same multiple of a magnitude $Z$; so that $LH:N=A:Z$. But we have $KH:LH=B:A$; hence, $KH:N=B:Z$; that is to say, $KH$ and $B$ are equimultiples of $N$ and $Z$ respectively. But $A$ is also a multiple of the same magnitude $Z$; therefore $Z$ is a common measure of $A$ and $B$.

Hence, if $LH$ is divided into parts that are each equal to $N$,

[1] Thus worded, the exact meaning of the last sentence of the paragraph is obscure.

and $A$ also into parts each equal to $Z$; then the number of parts in $LH$, that are equal in magnitude to $N$, will be equal to the number of parts in $A$, that are equal to $Z$. Hence, if to each of the parts of $LH$ there is applied one magnitude equal to Z, so that its 'weight-centre' is situated at the middle point of the part; all the magnitudes taken together will be equal to $A$; and the 'weight-centre' of the magnitude formed from all these magnitudes taken together will be the point $E$. For the whole set are even in number, on account of the fact that $LE = EH$.

Moreover, it can be shown in a similar manner that, if to each part of $KH$ there is applied a magnitude equal to $Z$, so that its 'weight-centre' is situated at the middle point of the part, all these magnitudes taken together will be equal to $B$, and the 'weight-centre' of the magnitude formed from all these magnitudes will be the point $D$.

Consequently, $A$ is, in effect, applied at the point $E$, and $B$ at the point $D$.

But, with this arrangement, we have a set of magnitudes all equal to one another, situated in a straight line, and their 'weight-centres' are equidistant from one another; also their number is even. It is plain, therefore, that the 'weight-centre' of the magnitude formed from all these magnitudes is the middle point of the straight line joining the centres of the two middle magnitudes. But, since $LE = GD$, and $EG = DK$, therefore the whole $LG =$ the whole $GK$. Hence, the 'weight-centre' of the magnitude formed from all the magnitudes taken together is the point $G$.

Therefore, $A$, applied at $E$, and $B$, applied at $D$, will balance one another at $G$.

Let us review the account given by Mach, in the light of the foregoing demonstration. First of all, even supposing that Archimedes had wished to give a pictorial illustration of Prop. VI, introduced maybe with some such words as, 'Just as in the case of a lever, we see, &c.', it is difficult to imagine him using a string and pulley. He would, I think, have been far more likely to have used a more delicate test, one that was free from all difficulties introduced by the friction of the pulley and the rigidity of the cord, and so on; namely, the method of suspending his first balance from one extremity of another balance, as in Fig. 8. Secondly, his figures give no hint of suspension to either a material

or a non-material lever ; they are simply supposed to be placed *at* certain points that are all in a straight line ; and although pictorially the effect of the string is to localize the point of application, yet the hanging weight introduces an idea that is quite foreign, and conveys a wrong impression. Hence, I consider that if Archimedes had wished to illustrate the condensation of the 'weight' at the balance-point, by means of the force on the fulcrum of a lever, he would have shown it in plan, as in Fig. 9.

We next come to the actual facts, as given in the words of Archimedes, which are wrongly represented by Mach. We note that Archimedes takes any straight line, divided in the required proportion, performs certain geometrical operations upon it, and proves certain facts about it and the two magnitudes under

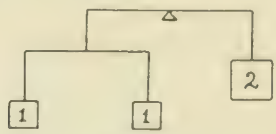


Fig. 8.

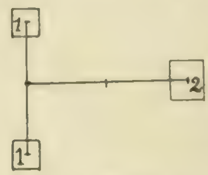


Fig. 9.

consideration, before ever there is any talk of loading the line. There is no hint as to any material difficulty in loading the line, such as would occur if the line were short and the magnitudes large. Thus there is no distinction made, such as can be comprehended from the figures given in Fig. 10, where for the same length of line we have different areas of rectangles as our magnitudes ; in the bottom figure, the parts cannot be 'laid' horizontally on the line. His idea evidently is that they are, so to speak, strung on the line, so that their centres coincide with certain points on it ; and there is no notion of any special disposition with regard to those centres. He *first of all loads his line uniformly*, the number of loads being determined by the particular common measure that he takes as that of the two segments of his original straight line. He then finds two equivalent loads to the uniform load ; and these are then stated to be equivalent to one another ; there is no replacing of the one principal load by the other, in the manner suggested by Mach.

Having thus criticized the explanation of Mach, I am bound to give a better explanation, if I can, for others to criticize. This I will endeavour to do, trying to make it match the reasoning of

Archimedes step by step. The foundation of the mathematical demonstration of a science must be certain perceptions, or fundamental principles, at least for the earliest investigator. Although these in some cases apparently are flashes of inspiration, in general they are not so ; what is termed the ' instinctive cognition ', in the case of such principles as those of ' symmetry ' or ' superposition of loads ', is merely the result of automatic or involuntary classification of all experiences that have impressed the brain, though maybe unnoticed at the time, nor indeed until their cumulative effect, like that of a cumulative poison, has become great enough. If we wish to explain this cognition to others, we

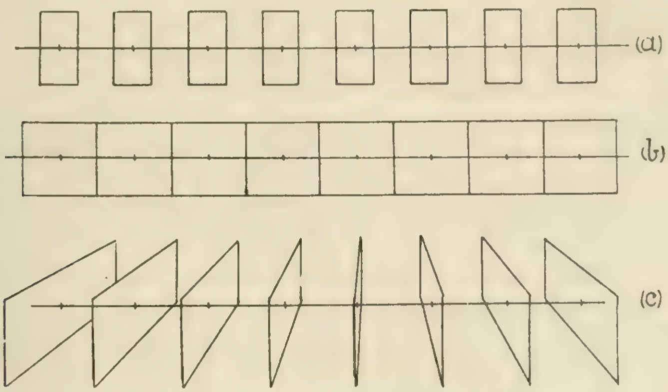


FIG. 10. Diagram (*c*) is drawn in perspective ; the other two can be considered as being in either plan or elevation.

must appeal to the experiences from which it has been derived. Thus, Archimedes' postulates and first three propositions are an appeal of this nature. It is just as if he said : You know what happens in the case of a balance, equal weights balancing at equal distances, and so on. Well, the same idea can be used in the case of straight lines and planes. Although these have no weight, we can imagine that, when a straight line is compared with a straight line, we can consider that it has, so to speak, a weight proportional to its length, and that this weight can be supposed to be condensed at its middle point, and it balances about this point. Similarly, in the case of planes, we can speak of such things as rectangles, and circles, when compared with things of the same nature as themselves, as having weights proportional to their areas, that these weights may be considered as being condensed at their balance-points, which are their geometrical

centres. With this idea, it is fairly clear that the 'weight-centre' of two equal planes or straight lines is the point about which they balance, that is the point about which they are symmetrically placed, *as far as their respective 'weight-centres' alone are concerned*; and so on. A modern teacher might illustrate Prop. V with a pack of cards, pinning each card by a pin through its centre to the appropriate point on the blackboard; and thus give his class a good idea of the principle of symmetrical disposition. In a similar manner, by an appeal to experience the principle of superposition of loads could be introduced, and applied to things having in reality no weight. Starting, then, with these preliminary notions, I suggest the following interpretation of the steps of the reasoning of Prop. VI, giving at the same time the steps from which Archimedes derived his general enunciation.

Suppose we start with an even number of equidistant planes or lines, say six, arranged with their centres all in a straight line, $A$, $B$, $C$, $D$, $E$, $F$. Then their weight-centre is their balance-point $G$, half-way between $C$ and $D$. Hence, if $H$ is any other point in the straight line, the whole six will balance, with respect to the point $H$, a plane, of six times the area, with its weight-centre at a point $K$, such that $HK = HG$; for the six planes can be supposed condensed at their weight-centre $G$, and would then balance six at $K$ (Fig. 11 $a$).

Again, the four magnitudes $A$, $B$, $C$, $D$, will balance four at a point $M_1$, where $HM_1$ is equal to the distance of $H$ from the middle point of $BC$; and the two at $E$, $F$ will balance two at $L_1$, where $HL_1$ is equal to the distance of $H$ from the middle point of $EF$. Hence, superposing the two loads, the whole six will balance, with respect to $H$, four at $M_1$ and two at $L_1$ (Fig. 11 $b$).

Similarly, the six will balance two at $M_2$, where $HM_2$ is equal to the distance of $H$ from the middle point of $AB$, and four at $L_2$, where $HL_2$ is equal to the distance of $H$ from the middle point of $DE$ (Fig. 11 $c$).

Now reverse the first balance, left to right, and superpose it upon one of the others, say on that given in ($b$); then we have the balance shown in ($d$). But we can remove the six pairs at equal distances; and the balance shown in ($e$) is left. But 6 at $G$ will be balanced by 6 at $K$; hence $K$ is the weight-centre, balance-point or centre of gravity of 2 at $L_1$ and 4 at $M_1$. The reasoning is general for any even number of equidistant equal

magnitudes. This is essentially the demonstration given by Archimedes, except that he shortens the proof, as soon as he has considered the equivalence in figures ($a$) and ($b$), by immediately stating that the two right-hand sides are equivalent to one another.

I submit that this is a more correct interpretation of the reasoning of Archimedes ; and also that this gives a more feasible explanation of the manner in which he arrived at his general proposition. That is to say, he did not reduce the harder cases

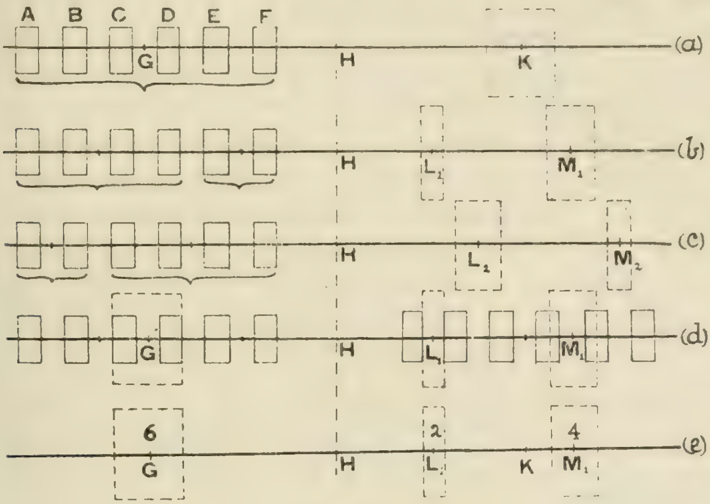


Fig. 11.

to the simple case ; but, from a general consideration of the general case of symmetrically placed magnitudes, *he was led* to the case of non-symmetry. I am convinced that, as a *mechanical idea,* the notion of ' moment ' does not enter into his reasoning. I will go even further ; I do not believe that, at the time of composition of the first book on Equiponderants, Archimedes would have admitted such a notion as the foundation of a rigorous mathematical demonstration. Further still, I do not believe that he *ever* attained to the idea embodied in the formula $\Sigma mx/\Sigma m$, as applied to finding the centroid.

For, in the first book of Equiponderants, after extending Prop. VI to incommensurables, he proceeds to find the centroid of a triangle. Here, he is forced to give a complicated indirect proof, because mathematical accuracy will not permit him to omit the triangles, by which a strip of a triangle cut off by two lines parallel to a side differs from a parallelogram, *no matter how*

*narrow that strip may have been taken.* Thus, instead of saying that the centres for all such strips lie on the median that bisects the side, and therefore the centre of the triangle lies on this median, he gives a proof of which the following is a brief account (see Heath's *Archimedes* for the full proof). That is to say, he does not feel justified in applying Prop. VI and VII except to magnitudes which have a *geometrical centre of point-symmetry*, such as a line, a rectangle, and a parallelogram (and it should be noted that for the latter there is not line-symmetry, which corroborates my contention that the essential idea of Archimedes was condensation of the magnitudes at their centres, irrespective of their disposition). Neither, at this stage at any rate, does he appreciate the idea of the infinitely narrow strip; or, at least, he refuses to admit it into accurate reasoning, although he uses it as a method of *discovery*.

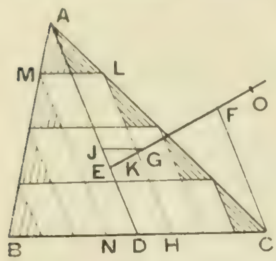

Fig. 12.

### Proposition XIII

If possible, let the centre of $ABC$ be at $G$, to the right of the median $AD$; draw $GJ$ parallel to $BC$ to meet $AD$ in $J$; divide $BD$, $DC$ into parts equal to $DH$, where $DH$ is less than $GJ$; complete the figure by drawing parallels.

Then the centre of the unshaded figure is on $AD$, at $E$ say. Join $EG$, cutting $LH$ in $K$, and a parallel to $AD$ through $C$ in $F$; this point must then lie completely to the right of the triangle $ABC$.

Now the ratio of all the shaded triangles to the whole triangle $ABC$ is as $AL$ to $AC$, i.e. as $EK$ to $EF$. But, since $G$ is the centre of the triangle, and $E$ that of the unshaded portion, then the centre of the shaded portion is at $O$, where

$$EG : EO = \text{shaded portion} : \text{whole triangle}$$

$$= EK : EF.$$

Now, since $EG > EK$, it follows that $EO > EF$; that is to say, $O$, the centre of the shaded triangles is still more to the right, than $F$, of the triangle; which is impossible.

Now the only reason, that I can see, for the employment of an indirect proof, is the rigorous manner in which Archimedes proceeds with his chain of propositions. For, if he allowed the mechanical idea to enter the reasoning, he has at his command

a proof by the method of exhaustions. Thus suppose we consider one of the strips of a triangle cut off by parallels to the base. Suppose *PQRS* (Fig. 13) is such a strip, and that *T*, *U* are the middle points of the parallel sides. Let the base *QR* be divided into an even number of small parts, *QX*, *XY*, . . . *LU*, *UM*, . . . *ZW*, *WR*; complete the figure as in the diagram, where, as in Archimedes' figure for Prop. XIII, *PX*, *KL*, *NM*, *SW*, are all parallel to *TU*.

Then *PQZS* is a parallelogram, *T* and *L* are middle points of opposite sides; hence its centre is in *TL*, i.e. to the right of *KL*; *hence considering* LT *as the knife-edge of a lever*, the whole strip *PQRS* will preponderate on the right, and its centre will be to the right of *TL*, i.e. still more to the right of *KL*. Similarly it is to the left of *NM*. Hence, the narrower the strip, i.e. the smaller the parts into which the base is divided, the narrower is the parallelogram which includes the centre of the strip.

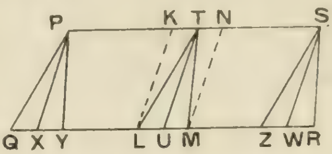

Fig. 13.

It follows by a continued application of the theorem that the centre of two magnitudes combined into one lies on the line joining the respective centres of the two magnitudes, that the centre of the triangle *ABC* in Prop. XIII lies to the left of *LH*, and to the right of *MN* in Fig. 12, *no matter how small the parts* ND, DH *may be*. Hence, it follows, by the method of exhaustions, that the centre lies on *AD*.

Now, this is distinctly the same train of reasoning that runs through the postulates and the first three propositions, before the transition is made to magnitudes; so that I am convinced that my suggestion that these are merely illustrative is the correct interpretation of them. That Archimedes, later at any rate, used the method of infinitely small strips, or, what comes to the same thing, the idea that an area is made up of parallel lines, and a solid of parallel planes, is proved by the palimpsest MS., discovered by Heiberg, of which a translation has been published by Heath, as a supplement to his *Archimedes*. A translation has also been given, in the *Monist* for April, 1909, by Miss Lydia G. Robinson, and this has been reprinted in pamphlet form and published by *The Open Court* as 'The Method of Archimedes'. It is to this pamphlet that I shall make reference in what follows, to

avoid reprinting the original propositions, as given by Archimedes. In the introductory remarks of this letter to Eratosthenes, Archimedes states (see p. 8, l. 11): 'much that was made evident to me through the medium of mechanics was later proved by means of geometry, because the treatment by the former method had not yet been established by way of a demonstration'. I will leave the consideration of the suggestions I have so far made to my readers, in the light of this pregnant passage: and I will pass on to my last point, that Archimedes does not use anything that can truly be said to be equivalent to a general moment theorem based on the formula $\Sigma mx/\Sigma m$, even in his 'Method' of discovery. To prove this most effectively, I will give modernized versions of the theorems in Miss Robinson's pamphlet (referred to as *R.M.A.*), and make remarks upon them as I proceed.

THEOREM I (compare with *R.M.A.*, p. 10).

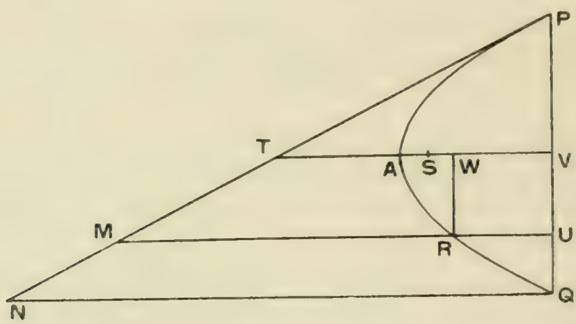


FIG. 14.

We have $4\,AS.RU = 4\,AS.(AV - AW) = PV^2 - RW^2 = PU.UQ$;
$4\,AS.MU = 4\,AS.TV.PU/PV = 2\,PU.PV$:
Hence $MU : RU = 2\,PV : UQ$, or $MU.UQ = RU.PQ$.
Summing, we have $\Sigma MU.UQ = \Sigma RU.PQ = \Sigma PQ.RU$;
i.e. (segment $APQ$) . $PQ$ = moment of $PQN$ about $QN$
$= 4.\Delta APQ\,.\,PQ/3$
$\therefore$ 3 (segment $APQ$) $= 4$ (triangle $APQ$).

Observe that the proof is valid for a non-symmetrical segment, if $PQ$ is taken parallel to the tangent at the vertex of a diameter $TV$.

THEOREM II (compare *R.M.A.*, p. 11).

We have (Fig. 15) $AE^2 + EG^2 = AG^2$, i.e. $EF^2 + EG^2 = EH^2$. Hence, multiplying by $\pi$, and summing, we have all the circles

of the cone formed by the rotation of $ACD$ together with all the circles of the hemisphere formed by the rotation of $ABC$ are equal to all the circles of the cylinder formed by the rotation of $ABDC$.

But the cone is one-third of the cylinder; therefore the hemisphere is two-thirds of the cylinder.

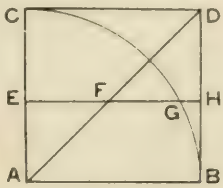


FIG. 15.

The first of these proofs is a mere version of the original; but the second is different altogether in form from the original. The method is so obvious, if the *summation* is admissible, that it would have been impossible for Archimedes to have missed it; but it does not lend itself to the method of balancing. Hence I suppose that it was not used owing to the infinite summation involved. The second point to be observed in the originals, as given by Archimedes, is that the lines in the first theorem and the circles in the second, are balanced in pairs; that is, Archimedes at this date uses nothing but the simple case of *two* balanced magnitudes, and then superposes loads; he makes no use of a general moment theorem. Finally, when he was investigating a quadrature, as in these theorems, his effort was to find a fourth proportional, if we use the mechanical idea for a moment, to (1) the distance, (2) a constant distance, (3) the weight, as represented by his line or circle; if the fourth proportional happened to work out as an elementary line or figure, of which he knew the dimensions, his success was assured. So that, here it is the *quotient* of the weight and the distance, and not the *product* that is in question.

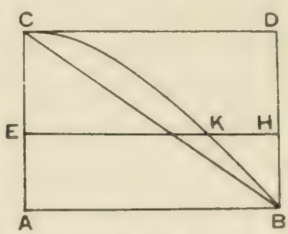


FIG. 16.

THEOREM IV (compare with *R.M.A.*, p. 15).

Since $CKB$ is a parabola, we have

$$EK^2 : EH^2 = CE : CA;$$

or $$EK^2 . CA = EH^2 . CE:$$

hence, multiplying by $\pi$, and summing, we have

$$\begin{aligned}(\text{vol. of conoid}) . CA &= \text{sum of moments of the circles of the cylinder about } CD\\ &= \text{moment of the cylinder about } CD\\ &= (\text{vol. of cylinder}) . \tfrac{1}{2}CA;\end{aligned}$$

hence vol. of conoid is half that of the cylinder.

But the vol. of cone is one-third that of the cylinder; hence the vol. of the conoid is one and a half times that of the cone.

There is but one point worth noting in this theorem, namely, that Archimedes quotes the volume of the cone in terms of the cylinder; just as in Theorem I he quotes the position of the centre of a triangle. The former of these reduces to the latter. For, in Fig. 17, if $EF' : EF = \pi . AB : AC$, we have

$$\pi . EF^2 = CE.EF' ;$$

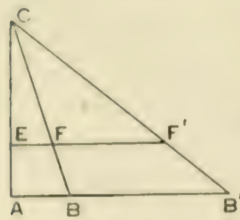


FIG. 17.

hence, summing, all the circles forming the cone are together equal to the moment of the triangle $ACB'$ about a parallel to $AB'$ through $C$, i.e. to the area $ACB'$ multiplied by the distance of its centre from $C$; from which we have

$$\text{vol. of cone} = \tfrac{1}{2}\, CA.AB' . \tfrac{2}{3}\, AC = \tfrac{1}{3}\, \pi . AB^2 . AC.$$

The question therefore at once arises: why, since Archimedes has demonstrated the position of the centre of a triangle, does he not give the volume of a cone as a corollary from it, as a sample of his method? Of course the answer may be that, as Archimedes says, the volume of the cone in terms of the cylinder had been enunciated by Democritos and proved by Eudoxus. I suggest, however, that the reason is far different; and that it is closely connected with my contention. Archimedes, as we see in Theorem I, is quite prepared to say that a triangle, 'where it is', is equivalent to its area condensed at its centre; *but he is not prepared to say that the triangle, where it is, is equivalent to the sum of the moments of its parts, or to the area condensed at the centre. when he is unable to 'balance' each of these parts separately against parts of another figure.* Apparently, there is no way in which this could have been done; and he is not prepared to adopt the infinite summation that is inherent in the general formula $\Sigma mx / \Sigma m$, when one side of his equation is a *pure summation* and the other a *summation of moments.*

THEOREM V (compare *R.M.A.*, p. 17).

Since, in Fig. 18, $APQ$ is a parabola, we have

$$PN^2 : QM^2 = AN : AM = LN : QM,$$

$$\text{i.e. } PN^2 : LN^2 = AM : AN ;$$

$$\text{or } PN^2 . AN = LN^2 . AM.$$

Hence, multiplying by $\pi$, and summing, we have

$$\Sigma\,(\pi\,.\,PN^2.\,AN) = \Sigma\,(\pi\,.\,LN^2)\,.\,AM.$$

Therefore, if $K$ is the centre of the conoid, we have

$$(\text{vol of conoid})\,.\,AK = (\text{vol. of cone})\,.\,AM\;;$$

$$\text{i.e. } AK : AM = 2 : 3,$$

for the conoid is one and a half times the cone.

In this theorem, the method is the converse to that used for Theorem I; here a centre has to be found by means of a known cubature, and the product of 'weight' and 'distance' is involved, since the circles of the given figure have to be kept 'where they are'. Still Archimedes balances the pairs of circles, *one pair at a time*, and does not use a general moment theorem.

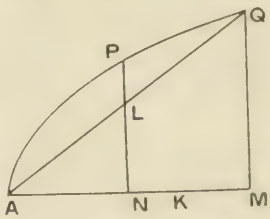

FIG. 18.

Archimedes might have determined the volume and the centre of the conoid at one and the same time, with a single figure, if he had so chosen; and, if he had done so, it would have been a most elegant example of his method. I cannot imagine why he did not

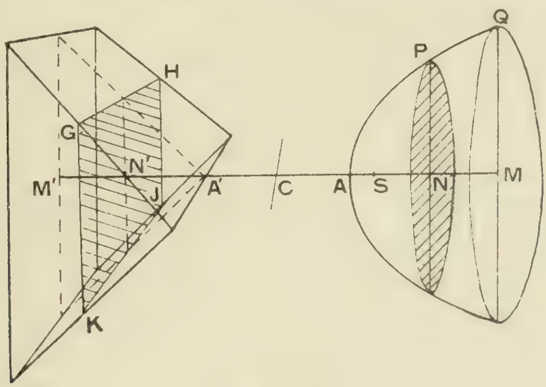

FIG. 19.

do so, except that, for the sake of clarity, the figure must be given in perspective.

Thus, in Fig. 19, if the breadth of the right-angled isosceles prism is $\pi$ times the semi-latus-rectum of the generating parabola, it is easily seen that there is a 'balance', *involving nothing but equal magnitudes at equal distances*, between the circles of the paraboloid and the rectangles of the prism.

For, the circle of radius $PN = \pi\,.\,PN^2 = 4\,\pi\,.\,AS\,.\,AN$; and the

rectangle $GKJH = GK \,.\, GH = 2A'N' \,.\, 2\pi \,.\, AS$; and these are equal if $AN = A'N'$.

Hence, the vol. of conoid = vol. of prism

$$= (\text{base of prism}) \,.\, \tfrac{1}{2}A'M'$$
$$= (\text{base of conoid}) \,.\, \tfrac{1}{2}AM$$
$$= \tfrac{1}{2} \text{ (circum-cylinder)}.$$

Also, since the two solids will balance at the middle point, $C$, of $AA'$, $NN'$, $MM'$, &c., and their magnitudes are equal to one another, their centres must be at equal distances from $C$. That is to say, the distance of the centre of the conoid from $A$ is two-thirds of $AM$.

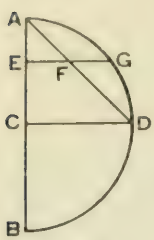


Fig. 20.

Theorem VI (compare *R.M.A.*, p. 19).

In Fig. 20, we have $EG^2 = AE.EB$,

hence $$EG^2 + EF^2 = AE.EB + AE^2 (= EF^2)$$
$$= EF.AB.$$

Multiplying by $\pi \,.\, AE$, and summing, we have

$$\Sigma\,(\pi \,.\, EG^2 .\, AE) + \Sigma\,(\pi \,.\, EF^2 .\, AE) = \Sigma\,(\pi \,.\, EF^2).AB\,;$$

i.e. (hemisphere).$AX$ + (cone).$AY$ = (cone).$AB$,

where $X$ and $Y$ are the centres of the hemisphere and the cone respectively.

Now, $AY = 3\,AC/4$, and the hemisphere is double the cone; therefore we have

$$2\,AX + 3\,AC/4 = 2\,AC$$
$$\text{i.e. } AX = 5\,AC/8.$$

In this theorem again, Archimedes has to consider the product of the 'weight' and the distance; but he evades the moment summation by balancing in pairs. He states that, if the circle whose radius is $EF$ is transferred to a point $K$ on $CA$ produced, such that $AK = AB$, it will balance the two circles together, whose radii are $EF$ and $EG$, where they are. There is another point about this theorem to be noticed, namely, that Archimedes does not complete the numerical part of the proof of his enunciation; he stops short at the determination of the balance between the solids. As the given figure contains two letters, designating the generating rectangles of two cylinders which are not referred to in the proof, it seems probable that there is here a hiatus in the manuscript, and the rest of the proof goes on as in Prop. VIII, which has

the same cylinders and the same letters to designate their generating rectangles. It will be useful to see how Archimedes treats the matter, *evading the infinite summation, and the reasoning of an algebraical character*; so I will now give a simplified version of Archimedes' own demonstration, merely eliminating to some extent the prolixity of the argument.

THEOREM VIII (compare *R.M.A.*, p. 21).

If, in Fig. 21, $EAZ$ is half a right angle, we have

$$PL^2+AP^2=AP.PC+AP^2$$
$$=AP.AC,$$

or $$PL^2+PO^2=AP.AT,$$ where $AT$ is taken equal to $AC$.

Hence, a circle with radius $PL$ together with a circle of radius $PO$ is to a circle with radius $PO$ as $AT$ is to $PO$ (or $AP$). Therefore

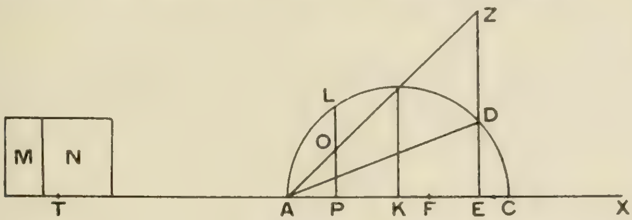


FIG. 21.

the spherical segment generated by $ALDE$ together with the cone generated by $AEZ$, where they are, will balance the cone generated by $AEZ$, set with its centre at $T$.[1]

Let $F$ be a point such that $AE=4\ EF$, then $F$ is the centre of the cone generated by $AEZ$. Let the cylinder generated by $M$ and $N$ together be equal in volume to this cone; also let the part of it generated by $M$, at $T$, balance the cone, where it is. Then the cylinder generated by $N$, at $T$, will balance the spherical segment, where it is. Hence (here I invert the argument of Archimedes for the purpose given in the footnote [2]), we have

$$\frac{\text{segment generated by } ALDE}{\text{cylinder generated by } N}=\frac{AT}{AK},$$

where $K$ is the centre of the spherical segment generated by $ALDE$.

[1] This corresponds to the point at which the demonstration of Prop. VI breaks off.

[2] This, I think, corresponds more with an *attempt to find* the ratio in question, than what is actually given by Archimedes, who *starts* the last part of the argument with the ratio, and then proves that $K$ thus taken is the centre.

Now, if $CX$ is equal to the radius of the sphere, it has been proved (*Sph. and Cyl.*, ii. 2, Cor.) that

$$\frac{\text{segment generated by } ALDE}{\text{cone generated by } AED} = \frac{XE}{EC};$$

but

$$\frac{\text{cone generated by } AED}{\text{cone generated by } AEZ} = \frac{ED^2}{EZ^2} = \frac{AE.EC}{AE^2} = \frac{EC}{AE};$$

also, since cone generated by $AEZ$ balances the cylinder generated by $M$, and is equal in volume to the two cylinders generated by $M$ and $N$ together, we have

$$\frac{\text{cone generated by } AEZ}{\text{cylinder generated by } M} = \frac{AT}{AF}, \text{ and } \frac{\text{cone generated by } AEZ}{\text{cylinder generated by } N} = \frac{AT}{CF}.$$

Hence, since $AT = AC$, by combining the ratios given, we have

$$\frac{AC}{AK} = \frac{XE}{EC} \cdot \frac{EC}{AE} \cdot \frac{AC}{CF}, \text{ or } \frac{AE}{AK} = \frac{XE}{CF}.$$

Now

$4\ FC = AE + 4\ EC$, and $4\ XE = 2\ AC + 2\ EC = 2\ AE + 6\ EC$;

therefore $\quad AE : AK = 2\ AE + 6\ EC : AE + 4\ EC$,

and $\quad AK : KE = AE + 4\ EC : AE + 2\ EC$.

Now compare the above demonstration with what follows, a proof which might have been given by Archimedes, if he had used the general moment theorem. Starting with the equality $PL^2 + PO^2 = AP.AC$, and multiplying by $\pi.AP$, and summing, we have, since $AP = PO$,

$$\Sigma (\pi \,.\, PL^2 .\, AP) + \Sigma (\pi \,.\, PO^2 .\, AP) = \Sigma (\pi \,.\, PO^2).AC,$$

or (seg. gen. by $ALDE$) . $AK$ + (cone gen. by $AEZ$) . $AF$
= (cone gen. by $AEZ$) . $AC$;

i.e. seg. gen. by $ALDE$ : cone gen. by $AEZ = CF : AK$. But,

$$\frac{\text{seg. gen. by } ALDE}{\text{cone gen. by } AED} = \frac{XE}{EC}, \quad \frac{\text{cone gen. by } AED}{\text{cone gen. by } AEZ} = \frac{EC}{AE}$$

and $CF : AK = XE : AE$; and the rest follows as before.

Now, it is seen that the introduction of the two cylinders generated by $M$ and $N$ have materially complicated the argument; and it stands to reason that Archimedes would not have introduced them except for a very good purpose. This purpose, it seems to me, is nothing else but the means of avoiding the

summation, which I have shown leads so easily to the required ratio $CF : AK$. The same thing is apparent in the next three theorems; and I should not give these, except on account of their extreme ingenuity, and because I think that the perspective figures I have used may enable my readers to follow Miss Robinson's pamphlet more easily, than is the case with Archimedes' figures which are in plan only. There is also another reason; the figures for the first two theorems in one case have the elementary planes across the line taken as the balance-beam, and in the other case along it; showing, as I have stated, that Archimedes is concerned with the centre only of these planes, and its position, and not at all with the disposition of his planes with regard to this centre. It may be, too, that this has something to do with the fact that the figures are given in plan only, for in the perspective view I have had to make them stand on edge on the line of balance; but I have done this only to avoid complicating the figure by introducing a parallel through the centres, and it does not militate against my argument. The reader must imagine that the letters of the proof refer to the parallel through the centre, which would be similarly divided to the base line; Archimedes, by giving only the plan, effects this automatically. The mention of the word 'balance-beam' above reminds me that I have not yet made a note of the fact that in these theorems Archimedes frequently makes use of the phrase 'Think of so-and-so as a scale-beam', according to Miss Robinson's translation; even here the language is figurative and employed for producing a better mental picture. In Prop. VI of the *Quadrature of the Parabola* we find a more or less distinct statement by Archimedes, in the following words:[1] '*Imagine* that which is proposed to be considered to be viewed in a plane upright to the horizon and the letters $AB$ (?); then *deem* points on the same side of $AB$ as $D$ to be *below*. . . .'[2] He *thereafter* goes on to speak of $AB$ as part of '*the* scale-beam' (*ζυγόν*), a term which is not used in the first book of Equiponderants. This shows very clearly that, at least in his own estimation, Archimedes, led to it by a consideration of the known properties of the lever, proves geometrically the principle of balance for planes and other magnitudes, and then uses the mechanical idea afterwards, and

[1] *Νοείθω δὲ τὸ, ὅτε ἐστὶ τὸ ἐν τᾳ̑ θεωρίᾳ προκείμενον, ὁρώμενον ἐπὶ ὀρθοῦ ποτὶ τὸν ὁρίζοντα καὶ τὰς* AB *γραμμάς· ἔπειτα τὰ μὲν ἐπὶ τὰ αὐτὰ τῷ* Δ *κάτω νοείθω.*

[2] The italics in this quotation are, of course, mine.

language derived therefrom, simply as a pictorial means of fixing the ideas.

THEOREM XI (compare *R.M.A.*, p. 23).

In Fig. 22, we have

$$\begin{aligned} \text{parallelogram } KE : \text{parallelogram } HE &= FK : FH \\ &= DO : ML \\ &= CD : CM \\ &= CN : CM, \end{aligned}$$

where $CN$ is equal to the radius of the cylinder.

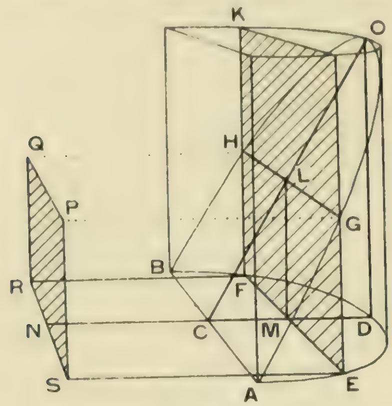


FIG. 22.

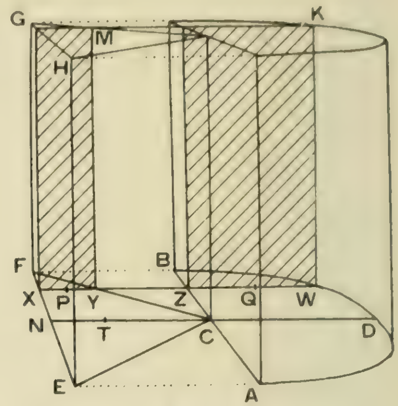


FIG. 23.

Hence, the parallelogram $HE$, if placed in the position $SRQP$, will balance at $C$ the parallelogram $KE$ where it is. Therefore the section $ABDO$, condensed at $N$, will balance the semi-cylinder where it is, at the same point $C$.

THEOREM XII (compare *R.M.A.*, p. 21).

The semi-cylinder on $ABD$ as base and the prism on $FEC$ as base can be cut by planes in corresponding parallelograms $ZK$, $XM$ (Fig. 23). Also there will be another pair on the other side of $NCD$ at an equal distance from it. . . .

(Here there is a hiatus in the manuscript; the reasoning can, however, be easily supplied as follows.)

Suppose that $P$ and $Q$ are the middle points of $XY$ and $ZW$ respectively. Then we have

$$2\,PZ = 2\,XZ - 2\,XP = AB - XY = AB - FX = AB - BZ = AZ;$$

hence, $\quad 2\,PZ.XY = AZ.ZB = ZW^2 = 2\,ZW.ZQ,$

and $\quad XY : ZW = ZQ : ZP.$

Hence, the parallelograms standing on $XY$ and $ZW$ are in the ratio of $ZQ$ to $ZP$ also; and, when combined with a similar pair on the other side of the diameter $NCD$, and equidistant from it, for the sake of symmetry, it is readily seen that there is a balance at $C$; and therefore also for the semi-cylinder and the prism.

Hence, if $T$ is the point on $CN$ immediately below the centre of the prism, i.e. the centre of the triangle $ECF$, we have

$$\text{vol. of section : vol. of this prism} = CT : CN = 2 : 3;$$

for the section, condensed at $N$, has been shown to balance the semi-cylinder, where it is; and it is therefore equivalent to the prism where it is.

Hence the volume of the section is one-sixth of the prism standing upon the square in which the base of the cylinder is inscribed.

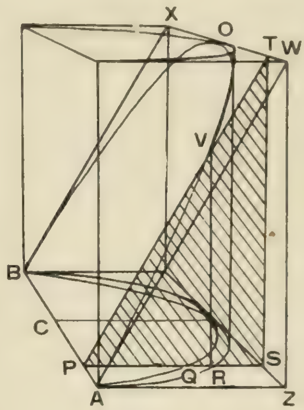

Fig. 24.

Theorem XIII (compare *R.M.A.*, p. 26).

If, in Fig. 24, $BQA$ is a parabola, $PR$ is a mean proportional between $PS$ and $PQ$.

Hence, $\Delta PST : \Delta PRV$

$$= PS^2 : PR^2$$

$$= PS : PQ.$$

Now, for all sections, the antecedents of the equal ratios are constant; hence the sum of the antecedent triangles is to the sum of the consequent triangles as the sum of the antecedent lines is to the sum of the consequent lines; that is, the cylinder-section is to the prism whose base is $AZW$ as the parabola $AQB$ is to the rectangle $ZB$, i.e. as 2 is to 3. Therefore the cylinder-section is one-sixth of the whole prism circumscribing the cylinder.

It is especially to be noticed, about the first two of these last three theorems, that in the demonstrations we have an exact equivalent to a change in the order of integration; the first summation being that of planes parallel to $AB$, and the second that of planes parallel to $CD$. When the form of the result has been obtained, the ratio 2 to 3 gives the clue to the parabola, and its ratio to the circumscribing parallelogram; and the geometrical proof is then fairly evident. In precisely the same way, the mechanical ideas of the postulates and the first three propositions

have given him the clue to the *idea of geometrical balance of magnitudes having no weight*; the proofs of the propositions on Equiponderants thereafter, starting with Prop. IV, are rigorously geometrical, although couched in terms reminiscent of their mechanical suggestion.

Now, what is effected in the balance proved in Prop. XII (p. 518) is the determination of the centroid of a semi-cylinder, or rather that of a semicircle; where the semi-cylinder and the prism are regarded as summations of equal and similarly situated planes perpendicular to each of the other sets given. For the proof really is that the triangle $FEC$ balances at $C$ the semicircle $ABD$; hence, if $V$ is the centroid of the semicircle, we have

$$\text{triangle} : \text{semicircle} = CV : CT,$$
$$\text{i.e. } CV : CD = 4 : 3\pi.$$

Lastly, we here see that Archimedes has the idea of a solid as the sum of planes, and a plane as the sum of lines, fully developed as a working notion, even if not as a means of rigorous proof. I take it that, in a similar way, he considers the line as a sum of *points*, and not as a succession of short straight lines; or else, surely, we should find in this connexion the determination of the centroid of a circular arc, which would give a most excellent example of the 'Method'.

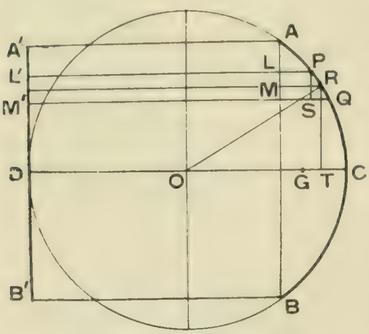

Fig. 25.

Thus, in Fig. 25, if the arc $AB$ is divided into a large number of parts, so that a part, $PQ$, can be considered as a straight line having its centroid at its middle point $R$, then, on account of the similarity of the triangles $PQS$, $ORT$ we have $PQ : OR = PS : OT$; and, therefore, the arc $PQ$, where it is, will balance the chord $LM$, transferred to $L'M'$. Hence, the arc $AB$, where it is, will balance the chord $AB$, transferred to $A'B'$; and, if $G$ is the centroid of the arc, we have

$$(\text{arc}).OG = (\text{chord}).(\text{radius}).$$

A neat modern version can be given by 'taking moments' about the diameter perpendicular to $OC$.

# ARISTOTLE ON THE HEART

By Arthur Platt

1. *Introduction.* It appears to me that there is room for a fresh examination of the Aristotelian description of the heart, that some of the problems and explanations of those problems previously offered may be set forth more coherently and clearly than hitherto, that a little common sense is all that is needed to clear away some of the difficulties, and, finally, that some portions of Aristotle's account may and should be interpreted in quite a new manner.[1]

In extracting his meaning it is often necessary to go behind the fragmentary and obscure wording of his statements and see what was really in his mind. The treatises of Aristotle are often of the nature of note-books for lectures; he puts down sentences intelligible to himself which he can amplify and make clear, but which by themselves are bewildering fragments. Besides this they have suffered terribly in the process of transmission to us, and are full of grievous blunders committed by scribes; whole passages have often fallen out and we can only guess what was in them; other bits have been added by people too ignorant to avoid supplying nonsense for sense. In fact when we come across a really thorny passage we can only deal with it if we have undergone a long and special training in Aristotelian scholarship which is an art by itself. And then most Aristotelian students, like the present writer, can lay no claim to be called anatomists. But perhaps it is an advantage in dealing with Aristotle not to know too much modern anatomy; one is more nearly in the position of an author who knew little enough himself from a modern standpoint.[2]

[1] I cannot help thinking that Professor D'Arcy Thompson despaired too soon in attacking this question in his translation of the *Historia Animalium* (iii. 3). I am truly sorry to find myself so often at variance with his notes upon the passage.

[2] Some years ago I submitted most of this paper to Sir George Thane; he disagreed with my actual novelties, and I put it aside and thought no more of it. Lately I disinterred it and on looking at it again was audacious enough to think

2. *Aristotle's Earlier Statement.* In the third chapter of *De Somno* Aristotle tells us that the finer and purer blood goes from the heart to the head, the thicker and more turbid to the lower part of the body. He tells us that the heart has three chambers, that the right is connected with the *vena cava*, the left with the *aorta*, while the middle chamber is 'common to' the other two, i.e. communicates with both, and separation of the blood into finer and thicker is effected in this middle chamber. But he adds that details must be added later, for 'to distinguish about these things is more fitting for other discussions'.

When we look at the 'other discussions' we find a totally different theory: it is much more elaborate and shows that its author had investigated the heart carefully. I think, therefore, we must assume that the statement in *De Somno* is the earlier, and that Aristotle completely changed his views on the question later on, for it is not credible that he should have first developed so minutely the account given in the *Historia Animalium* and have then contradicted it by the short and cursory account of the *De Somno* with the added observation that you must look elsewhere for details, knowing all the time that the details were quite different. At the same time it is not a necessary conclusion that the *De Somno* as a whole is earlier than the *Historia Animalium* and the *De Partibus* as wholes. It is more probable that Aristotle kept on working at all these treatises alongside of one another, and it is not possible to arrange the completed results in order of date. Moreover, when he had changed his views on any particular point, he did not always cancel the discarded earlier statement. Hence there are many contradictions in his writings.

3. *Aristotle's Later Statement.* In the *Historia Animalium* [1] he says: 'All hearts contain chambers, but in very small animals even the largest is barely visible, in animals of intermediate size the second also may be seen, in the biggest all three. . . . The largest chamber is on the right and highest up, the smallest on

that I was in the right in my conception of what Aristotle meant. I showed it therefore to Professor Elliot Smith and Dr. Singer, who encouraged me to publish it. Sir George's criticism has caused me to improve my nomenclature in several places, and I am grateful to him for that and for his patience and kindness in discussing the whole question with me. I know that this note will look sufficiently ridiculous, but it is only in interpreting desperately difficult Greek that I venture to dispute his verdict.

[1] iii. 3, 9, 10.

the left, that which is intermediate in size is between the two; both are much smaller than the largest.' He also says that the aorta springs from the middle chamber, a very important difference from his earlier statement which we shall presently discuss. And he says not a word about the middle chamber communicating with the other two, nor about the blood being differentiated in it. Instead of that he substitutes a new theory, that all three chambers communicate with the lungs. I shall presently translate and comment on the whole passage, but these are the most important points for our present purpose.

At the same time in the *De Partibus Animalium*,[1] arguing that large animals have more cavities than small ones, he says: 'And it is still better that there should be three chambers, in order that there may be one common principle or origin; now the middle and odd is a principle or origin.' In the same chapter Aristotle definitely contradicts the earlier statement about the blood being differentiated in the central chamber; it is better, he now says, that it should be kept distinct in the right and left chambers.

4. *Galen's Explanation.* According to Galen,[2] Aristotle's 'third cavity is that at the broad end of the heart, being part of the right chamber, not a distinct third chamber, and there runs from it a vein (φλέψ) into the lung, whereof the tunic is the same as that of the artery'. That is to say that Aristotle, in Galen's opinion, made the mistake of supposing the right ventricle to be two distinct chambers instead of one, though it is truly hard to see how he could have fallen into so gross an error. Aristotle's 'third cavity', then, in Galen's view, is the upper part of the right ventricle, his middle cavity is the lower part thereof, and the remaining one is the left ventricle.

It is palpable that this explanation does not suit the later statement of the *Historia Animalium*, but if we apply it to the earlier statement of the *De Somno* it is easy to see why Galen proposed it. If, as Aristotle asserted in *De Somno*, the right and left chambers were attached to vena cava and aorta respectively while another chamber lay between them, then what Galen calls the 'third cavity' could hardly be anything else than what Galen supposes, however gross the error imputed to Aristotle may be. The mention of the 'vein whereof the tunic

[1] iii. 4, 24.

[2] *De venarum arteriarumque dissectione*, cap. ix. Kühn, vol. ii, p. 817.

is the same as that of the artery ' (φλὲψ ἧς ὁ χιτὼν ὁ αὐτός ἐστι τῷ τῆς ἀρτηρίας) leaves surely no doubt about Galen's meaning the upper part of the right ventricle, and in this explanation he omits the auricles altogether. The ' vein ' in question is of course the pulmonary artery, which was regarded as a vein by the early anatomists (*vena arterialis*) because it is derived from the right side of the heart and appears from its position to belong to the venous system.

But it altogether passes comprehension to understand why Galen should ignore the vastly superior theory of the *Historia Animalium*. If, however, you suppose that Galen really thought he was explaining *that*, you must admit that he made a complete failure of it, and that it is an extraordinary accident that his attempt to explain one statement accounts for another. At the same time Galen is not entirely to be trusted in his comments upon his predecessors ; he sometimes refused to look facts in the face when they seem derogatory to any ancient whom he respects. Thus he endeavours to save Plato's credit on the absurd theory (which Plato took from the Hippocratic school) that what we drink goes into the lungs, and his defence of this is disingenuous. So it is of course possible that he was talking somewhat at random about the third Aristotelian chamber.

5. *Vesalius's Explanation.* Vesalius thought that Aristotle was misled by ' the membrane of the left ventricle ', i.e. the flap of the mitral valve, into thinking that this ventricle consists of two separate chambers.[1] He thus agrees with Galen in accusing Aristotle of splitting up one chamber into two, but he differs from Galen in saying that he splits the left ventricle instead of the right and he gives a plausible reason to account for the mistake. But above all he is trying to explain the later and developed account of the *Historia*, and escapes the strange error into which Galen fell when he treated the earlier account as if it were a fair representation of Aristotle's opinion. According to Vesalius, then, what is now called the aortic vestibule is Aristotle's middle chamber, and the rest of the ventricle is the left chamber. Therefore this theory will account so far for the later statement. But it certainly is hard to see how the flap of the valve could have so misled Aristotle, and there is a very great difficulty in the way besides. For Aristotle distinctly says that the *left* chamber is the

[1] *De fabrica corporis humani*, Lib. VI, cap. 12.

smallest, and on the Vesalian explanation the *middle* is much the smaller of the two. Consequently this theory must be rejected; it does not suit either the earlier or the later statement.

6. *The Auricular Theory of Huxley and Ogle.* To explain the later statement we are thrown back upon the theory advanced independently by Huxley[1] and by Ogle.[2] The left chamber, according to them, is the left auricle, the middle chamber is the left ventricle. This accounts for the details of the later statement perfectly so far as we are concerned with them for the present, and I hope to show that it will account for those we shall meet with later on. If the left chamber is so small a thing as the auricle, that is why Aristotle could not make it out in 'animals of intermediate size'. If the left ventricle is the middle chamber, that is why Aristotle says the aorta issues from the middle chamber. And should it be objected that he says his middle chamber is much smaller than the right one, the answer to this is that it really would appear to be so in an animal which had been suffocated. And the right auricle is entirely ignored by him because in a suffocated animal it would appear to be part of the vena cava.

7. *The Text of the later Statement reconsidered.* We may now turn again to the passage in the *Historia Animalium.*[3] 'The investigation is difficult, as has been said; it is only possible to acquire adequate information, if any one takes an interest in such things, in the case of animals suffocated after previous starvation.'

Aristotle made his dissections therefore on the bodies of strangled animals. We know what he would find. The right auricle and ventricle and the pulmonary artery would be gorged with dark blood; the left auricle and ventricle, the pulmonary veins, and aorta, would be empty. Therefore the right part would be easily made out; the left would have collapsed and be difficult to make out.

'The following is the nature of the blood-vessels. There are two vessels in the thorax, in the neighbourhood of the spine but lying within this[4] [i.e. in front of it], the greater in front and the lesser behind the greater. And the greater is more on the right, the less on the left; this latter some call aorta or suspender because the sinewy part of it has been observed even in the dead.'

[1] T. H. Huxley, *Nature*, November 6, 1869, republished in *Science and Nature*, p. 180.

[2] W. Ogle, *Aristotle on the parts of animals*, London, 1882, pp. 197–9.

[3] iii. 3. 6–14.

[4] I read ῥάχιν μὲν ἐντὸς δὲ κείμεναι ταύτης.

The greater vessel is of course the vena cava, both superior and inferior, the two (with the intervening right auricle) being treated as one by Aristotle as they were later by Vesalius in his *Tabulae sex*. The aorta is so called because it is sinewy like a strap or ' suspender ' of a wallet or the like,[1] and the heart seems to hang from it. This sinewy part has been observed ' even in the dead ', that is to say, even when the vessel has been exposed in a man killed by a sword-stroke or something of the kind, without the previous starvation and the suffocation which Aristotle considers rightly to make investigation easier.

' These vessels ', Aristotle continues, ' have their origin in the heart. For whereas they preserve their character unimpaired and remain vessels when they pass through the other organs, wherever they actually do so run, the heart on the contrary is as it were a part of them. This is more especially so with the vena cava, but applies also to the aorta, both vessels taking both an upward and downward course and the heart being in the middle of them.'

Aristotle had, of course, no idea that the arteries and veins anastomosed at their terminations. When he says that they ' pass through the other organs ' he has been misled into thinking that they do so, because they would be hidden by fat, &c., and would look as if they did. He adds, ' Where they actually do so run ', or ' Where they happen to do so ', which shows that he knows it to be exceptional. The main point here is that the heart is like an enormous dilation of the vessels and of the same sort of substance, whereas the other organs are bodies through which a vessel passes (if at all) as a foreigner through a strange land. His medical predecessors had said the vessels had their origin in the head !

' All hearts ', he proceeds, ' contain chambers, but in very small animals even the largest is barely visible, in animals of

[1] This was correctly explained by Vesalius : ' Magna igitur arteria (quam Aristoteles eo quod nervosam ipsius partem vel in mortuis conspici posse diceret, instar vaginae fortassis Macedonibus ἀορτῆς nuncupatae, ἀορτήν appellavit) . . .' *Corp. Hum. Fab.*, Lib. III, cap. 12. Only he is wrong in supposing that Aristotle invented the name. *Aorta* has no connexion whatever with *air* ; Aristotle had never even heard of the notion that arteries contain air, which notion was hatched in the next century by Erasistratus.

intermediate size the second also may be seen, in the biggest all three. . . . The apex of the heart points forward, as already observed, and the largest chamber is on the right and highest up, the smallest on the left, that which is intermediate in size is between the two ; both are much smaller than the largest.

'All these are connected with the lung by perforations, but this is not plain except in the case of one of the chambers, because of the smallness of the passages. [The connexion of the largest chamber with the lung is this] : the vena cava is attached to the largest chamber, that which is uppermost and on the right, then through the middle of the cavity extends again a blood-vessel [to the lung], as if the chamber were a part of the vessel in which part the blood forms a lake. The aorta [springs] from the middle chamber [and is also connected with the lung] only not in the same way but communicates [with it] by a narrower air-passage (syrinx).'

This last paragraph has been, I believe, hitherto entirely misunderstood, owing to the exceeding obscurity of connexion which is unluckily characteristic of the writer ; I also suspect that some words have been lost. Those in brackets have been here added to make the connexion plain. The last sentence without that addition is utterly unconstruable, or at any rate utter nonsense.

Aristotle held that the lung existed for the purpose of bringing the air and the blood into touch with one another. He also held that blood was formed in the heart ; I cannot quote any passage to show positively that he thought this was done in *all* his three chambers independently of one another, but there can be no reasonable doubt, I suppose, that he did think so. He then is anxious to insist that the lung is connected with all three chambers and so with the blood all over the body. Thus he lays stress first on the connexion of vena cava, heart, pulmonary artery and lung, and that much is to me plain. Next comes the left ventricle, from which springs the aorta, and the aorta too is connected with the lung by a narrower *syrinx.* This word in Aristotle is always used of air-passages ; Hippocrates uses it for a fistula, but I do not find that it is ever used for a blood-vessel. In this particular sentence the idea that it means a blood-passage of some kind has led to mistranslation and confusion ; it was the consideration of this word more than anything else which led me to my view of the meaning of the whole sentence,

and no explanation can be considered reasonable which takes *syrinx* here to mean a blood-vessel.

But if the *syrinx* is not a blood-vessel and if it connects the aorta in Aristotle's opinion with the lung, what is it? Aristotle was anxious to show that the aorta connected with the lung. Now there *is* a passage which joins the aorta not far from its starting-point, the ductus arteriosus; in the foetus this is a blood-vessel connected with the pulmonary artery, but in the adult it is closed up, and forms the ligamentum arteriosum. An early anatomist exploring the thorax without the light of previous experience or older records would not be likely to make out the real connexion, but he would be not unlikely to come upon this ligament in attempting to disengage the aorta. Suppose that Aristotle did this: he was looking for his connexion with the lung, he found this ligament broken off and jumped to the conclusion that it was the connexion he wanted. This is no unworthy accusation against him; he did sometimes find what he looked for when it was not there, as many have done since. But there was something peculiar about the passage, and not being obviously a blood-vessel he called it a *syrinx*; had it been more manifestly a blood-vessel he would have called it *phleps.* That it is not a hollow tube is true, but this is no serious objection, for he also calls the optic nerves 'passages' (πόροι), doubtless thinking that incapacity to see a real 'passage' in such things with the naked eye did not prove their non-existence.

He cannot have meant one of the bronchial arteries, because they would have been called by him φλέβες.

The word for word translation of the passage runs thus: 'But the aorta from the middle only not so but by a much narrower syrinx communicates.'[1] Consider the words *not so, syrinx, communicate.* If we suppose the meaning to be that the aorta 'communicates' with the heart, why should he use such a verb at all? Elsewhere he speaks of the aorta as 'having its origin in the heart' or 'running from the heart' or some such phrase. As a matter of Greek, κατὰ σύριγγα κοινωνεῖ cannot mean 'joins (the heart) by an aperture', which it is commonly supposed to mean; the syrinx cannot possibly be identified with the aorta itself or the aperture thereof; it must mean some other 'pipe', and that an air-pipe. The aorta therefore springs from

[1] *Historia Animalium*, iii. 3, $513^b$ 6.

the middle chamber and communicates with something else by some air-passage which is distinct from the aorta. Aristotle is plainly thinking, as it seems to me, of the connexion of heart and lung. And 'not so' is intelligible in my translation, but not in any other.[1]

Thus the two last sentences in the second paragraph on p. 527 contrast the connexion of vena cava and aorta with the lungs; the former communicates by the pulmonary artery which is a prolongation of the vena cava *through* the right ventricle, the latter not by sending off a similar branch through the left ventricle (*not so*) but by the ligament which is *much narrower* than the pulmonary artery.

There still remains one sentence of this paragraph, in which I accept the reading and interpretation of the great majority of commentators: 'But whereas the vena cava runs *through* the heart, the aorta runs *away from* the heart.'[2] This is an insistence again upon the same thing and needs no further comment. But we should expect now to hear how the third chamber, the left auricle, is connected with the lungs. Of course it would be by the pulmonary veins. But instead of going on to this, Aristotle proceeds to point out other differences between vena cava and aorta, and to explain the distribution of the venous and arterial systems over the body. Though he more than once asserts that all his three chambers are connected with the lungs he nowhere explains all three connexions together. But a passage elsewhere in the *Historia Animalium*[3] shows us clearly what he meant. 'The passages from the heart (blood-vessels) lie above (the air-passages in the lungs); there is no common passage (i.e. no opening by which air and blood can mix), but the vessels receive the air

[1] The only instance of σῦριγξ in the sense of a blood-vessel of any kind is, I believe, in a line of Apollonius Rhodius (iv. 1647), wherein a fabulous bronze monster is said to have had a σῦριγξ αἱματόεσσα. It would take a bold man to argue from a solitary poetic use against the established use in the medical and scientific writers; an ornithologist cannot ascribe a 'fiery heart' to a bird because a poet chooses to do so.

Empedocles spoke of some sort of σύριγγες λίφαιμοι, but this very epithet means 'bloodless', and nobody can tell what Empedocles meant. It cannot be denied that Aristotle supposed him to refer to φλέβες, but neither can it be denied that Aristotle must have misinterpreted him; he writes a horrible poetical jargon which is often as obscure as *Sordello*.

[2] *Historia Animalium*, iii. 3, 513$^b$ 7.

[3] *Historia Animalium*, i. 17, 496$^a$ 30–3.

on account of their apposition (i.e. through the walls of the air-passages and vessels) and transmit it to the heart. For one of the passages (pulmonary artery) carries it into the right cavity, *and the other* (*pulmonary veins*) *into the left.*' This second passage must be the pulmonary veins, not the aorta, because according to the *Historia Animalium* the aorta belongs to the middle cavity. And this *left* cavity here, which thus is connected with the lung by a passage corresponding to the pulmonary artery can surely be nothing whatever except the left auricle. This indeed seems to me the strongest proof to be had of the correctness of the auricular theory.

In the chaotic state in which Aristotle's works have come down to us, for whatever reason, it is not to be wondered at that this explanation should be here omitted. There are plenty of other like omissions in him. The passage continues :

§ 13. 'And the great vessel is membranous and skin-like, but the aorta is narrower than it and very sinewy ; as it stretches away further towards the head and the lower parts it becomes altogether narrow and sinewy.

§ 14. 'There runs first upward from the heart a part of the great vessel towards the lung and the junction of the trachea (reading with Karsch ἀρτηρίας for ἀορτῆς), a great and undivided vessel (i.e. the main channel of it does not bifurcate into equal halves but as a whole it may be called undivided), but still there do split off from it two branches, the one to the lung (i.e. pulmonary artery, for as we have seen this is regarded as a prolongation of the vena cava through the heart), and the other to the spine and the last cervical vertebra (i.e. the azygous vein).

§ 15. 'As the lung is double, so the vessel which runs to it first bifurcates and then runs along each air-channel and each perforation.'[1]

When we look at §§ 14 and 15 together we can hardly doubt that Ogle is right in his interpretation of these two branches as pulmonary artery and azygous vein. But the way in which Aristotle puts his statement is very confused. For he first says that the vena cava runs up from the heart *towards* the lung, and then says it begins by sending off a branch to the lung. Now the branch which it sends off to the lung, if it is the artery, goes off before the main vena cava starts upwards from the heart on its

[1] *Historia Animalium*, iii. 3, 513$^b$ 7–9.

principal course, and Aristotle knew this quite well, for he has just explained it all. We must fall back upon the fact that his works are of the nature of an ' open lecture-book ', in which are jotted down additional notes, often leaving earlier views not entirely obliterated but showing through a later stratum imposed upon them. We have here a jumble of this kind, but it is possible to see what he really meant.

With regard to the words ' towards the lung and the junction of the trachea ', I feel sure that this is the right reading. The texts say ' the junction of the aorta ',[1] but this means nothing. Junction with what ? How can the vena cava superior, the ' undivided vessel ' which is *not* the pulmonary artery, have any connexion with the aorta ? Substitute therefore *arteria* for *aorta*, (and *arteria* in Aristotle means the trachea). The junction of this, taken in connexion with the context, obviously means the place where the trachea splits up into the lungs. He is thinking of the course of the vena cava superior as an undivided vessel up to the point where it splits into the innominate veins, and this point is in the region where is what he calls ' the junction of the trachea '.

There is still one passage on which I have something to say. After going on to describe the vessels of the lungs and the azygous vein, Aristotle continues in § 17 : ' This is the way in which these branches are split off from the great vessel. But above these ἀπὸ τῆς ἐκ τῆς καρδίας τεταμένης the whole vessel again divides into two regions ', i.e. the vein does not merely give off a vein this time but divides as a whole into two equal parts running to two distinct regions. The words I have left in the Greek are unconstruable. Read instead ἀπὸ τῆς (or ἐκ τῆς) καρδίας τεταμένη. Then the meaning is ' the whole vessel as it lies stretched from the heart ', and this clause is added to show that, after the digression about pulmonary artery and azygous vein, we are going back to the vena cava itself, which as already said runs upward from the heart.

[1] It was this unhappy corruption of the text that drove Ogle into supposing that Aristotle was talking in this place of the ductus arteriosus, an impossible notion, because (1) he examined adult animals as he has told us himself ; (2) it is the ' great undivided vessel ' of the vena cava which is said here to run up to the ' junction ' and that ' undivided vessel ' is carefully distinguished here from the pulmonary artery.

With the whole description it is interesting to compare the following account from a work attributed to Hippocrates, but of uncertain date and authorship. I have emended the Greek, which is unintelligible in the standard editions.[1] 'The original vessel which, running along the spine and through the region behind the throat and the bronchus is fixed into the heart, sends off from itself a good-sized vessel with many mouths at its point of leaving the heart (!) and thence makes a *syrinx* through the lung into the mouth, which is called *arteria* (i.e. the trachea).' Such is angiology before Aristotle! Like other Hippocratics the author starts his blood-vessel from the head, whence it comes down to the heart and sends off the pulmonary artery: here again that artery is regarded as a branch of the vena cava. Then this runs through the lung and issues from it as the trachea, which is thus itself a prolongation of the vena cava. But again we find that the word *syrinx* turns up as soon as ever the author gets plainly into touch with an air-passage.

[1] *De ossium natura*, cap. 5; Littré, ix. 171; Kühn, i. 514.

# APPENDIX TO ARTICLE X

[*For reference in text see pages 410 and 411*]

Translation from the Dutch by Miss M. Kuenen of a passage in the *Œuvres complètes de Christiaan Huygens*, vol. xiii, fasc. 1, p. 591, The Hague, 1916.

'Copy of the copy exhibited by Adrian Van der Wal, 1682, whence it appears that Jacob Metius was not the inventor of the Telescope but rather Lippershey of Middelburg.

To their Honours, the States-General of the United Netherlands.

Jacob Adriaenssoon, son of Mr. Adriaen Anthonissoon, ex-burgomaster of the Town of Alkmaer, deposes that for a period of two years, during the time remaining over from his trade, he has been occupied in searching out such hidden arts as had been attained by predecessors in the use and application of glass. He has come to the conclusion, by experiments with a certain instrument which he was using for another purpose, that the sight could be extended to such a degree that things invisible or indistinct by reason of distance, could be clearly seen by its aid. When he had observed this he experimented to improve it further, and he progressed so far with his instrument that a thing can be seen as far off and recognized as clearly as with the instrument recently shown your Honours by a citizen and optician of Middelburg, according to the judgement of His Excellence [*i. e. Prince Maurice*] and of others who have experimented with the respective instruments one against the other.

Although the Suppliant's instrument is made of very poor material and for one experiment only, yet he does not doubt that it would greatly improve with better materials. Moreover, he also expects and hopes so to improve the invention itself so that more useful results may be expected. But he fears that meanwhile some one may anticipate him by copying or imitating his instruments, building on the foundations which he (by God's grace) has laid with his own skill, industry, and thought and thus deprive him of the fruits that he may justly expect therefrom. He therefore humbly begs your Honours to grant him a patent, whereby all who have not previously had or used this invention be prohibited from copying it entirely or in part, or from buying or selling such instruments made by unlicensed persons without the special consent of the Suppliant, at risk of confiscation of such instruments plus the sum of 100 guilders for each.

Finally, for twenty years, or as otherwise agreed upon, to render the Suppliant in respect of the use and service of the instrument for the Commonwealth, such honours as your Honours, with your usual goodwill and discretion, shall decide.

In margin was written that the Suppliant was admonished to investigate further to bring his invention to the greatest perfection, and suitable steps shall be taken about his request for a patent.

Enacted Aersens, October 17th, 1608.

[*Aersens was Secretary to the States.*]

After comparison, this and the original request signed by his own hand, were found to coincide word for word.

In Alkmaer the 8th Nov. 1677. Quod attestatur Joh. H. Metius 1677.'

[*This Joh. H. Metius of 1677 is, naturally, not the same as the Jacob Metius of 1608 whose invention is in question.*]

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